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THE PROFILE OF MATURITY IN ALPINE GLACIAL EROSION.[1]

THE literature of glaciation has not escaped the blemish of too free generalization. It was early asserted of the Sierra Nevada, for example, that Pleistocene glaciers of the alpine type, descending from an ice-cap in the summit region of the range, had reached to the range foot, and that the abnormally large canyons, particularly of the western flank, were the products, from head to foot, of glacial erosion. Such a statement made of southeastern Alaska, of the Scandinavian peninsula, or of the Patagonian Andes would not on the face of it be absurd. Nor was it absurd of the Sierra Nevada. It was possible, despite the low latitude, that ice-streams should have descended to the range foot, and it was theoretically not impossible that they should have excavated deep canyons. The matter, especially of glacier efficiency in erosion—a vexed question—is one mainly of the evidences. It cannot safely be handled deductively; and it need not be, since in glaciated mountains the evidences crowd the field. But the announcement was unscientific, because unsupported by facts of observation. Its author had no right to make it. On the other hand, it was no less unwarrantable and dogmatic to assert the contrary, which also was freely done.

My own acquaintance with the phenomena of glaciation of the alpine type had its beginning in the Sierra Nevada, in 1883, in the latitude of the Yosemite Valley—the so-called High Sierra. Prevailing opinion as to that region, it appeared, ranged between the

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two extreme views indicated; namely that, as regards quantitative effects in degradation more especially, glaciation had been widely destructive of the preglacial topography, on the one hand; on the other, that it had been relatively protective. But there was no recognition of distinctive forms—beyond "U-canyons" and moraines. I had little notion, therefore, as to what I should discover; only an open mind and a lively curiosity.

I was a maker of topographic maps, of some experience, and had a topographer's familiarity with the erosion aspects of mountains; but only of unglaciated mountains I had as well, however, something of the inquisitiveness of the physiographer as to the origin and development of topographic forms.

The first station occupied in this work of survey was Mount Lyell, one of the most widely commanding summits of the vast mountainous tract of the High Sierra.

From Lyell there was disclosed a scheme of degradation for which I had not been in the least prepared. No accepted theory of erosion, glacial or other, explained either its ground-plan outlines or its canyon-valley profiles; and, so far as I can see, none makes intelligible its distinctive features now. The canyons, at their heads, were abnormally deep; they were broadly flat-bottomed rather than U-formed, the ratio of bottom width to depth often being several to one; and their head walls, as a rule, stood as nearly upright, apparently, as scaling of the rock would permit. I characterized them, figuratively, as "down at the heel." In many instances the basin floor, of naked, sound rock in large part, and showing a glistening polish on wet surfaces, was virtually without grade, its drainage an assemblage of shallow pools in disorderly connection; and not infrequently the grade was backward, a half-moon lake lying visibly deep against the curving talus of the head wall, and visibly shallowing forward upon the bare rock-floor.

The amphitheater bottom terminated forward in either a cross-cliff or a cascade stairway, descending, between high walls, to yet another flat. In this manner, in steps from flat to flat, commonly enough to be characteristic, the canyon made descent. In height, however, the initial cross-cliff at the head dominated all. The tread of the steps in the long stairway, as far as the eye could follow, greatly

lengthened in down-canyon order. In that order, also the phenomena of the faintly reversed grade and of the rock-basin lakes rapidly failed. Apparently, at the canyon head, the last touch of vanishing glaciation had been so recent that filling had not been initiated, while down-stream, incision of the step cliffs and aggradation of the flats had made at least a beginning in the immense task of grade adjustment; the tread of the step was graded forward, but so insensibly, as a rule, that its draining stream lingered in meanders on a strip of meadow, as though approaching base-level. These deep-sunk ribbon meadows, still thousands of feet above the sea and miles in length, reflecting in placid waters their bordering walls or abnormally steep slopes, presented an anomaly of the longitudinal profile in erosion no less impressive than that of the upright canyon heads.

In ground plan, the canyon heads crowded upon the summit upland, frequently intersecting. They scalloped its borders, producing remnantal-table effects. In plan as in profile, the inset arcs of the amphitheaters were vigorously suggestive of basal sapping and recession. The summit upland—the preglacial upland beyond a doubt—was recognizable only in patches, long and narrow and irregular in plan, detached and variously disposed as to orientation, but always in sharp tabular relief and always scalloped. I likened it then, and by way of illustration I can best do so now, to the irregular remnants of a sheet of dough, on the biscuit board, after the biscuit tin has done its work.

In large part, apparently, a preglacial summit topography had been channeled away. By sapping at low levels, by retrogressive undercutting on the part of individual ice-streams at their amphitheater heads in opposing disorderly ranks, the old surface had been consumed, leaving sinking ridges, meandering dulled divides, low cols or passes, and passageways of transection pointing to piracy and to wide shiftings of the glacial drainage. There was not wanting a scattering of the more evanescent sharp forms of transition which the hypothesis would require, as thin arêtes, small isolated table caps, needle-pointed Mätterhorn pyramids with incurving slopes, and subdued spires (in the massive granite tracts) with radiating spurs inclosing basin lakes. The broader areas of this deep erosion, where

complex channeling seemingly had passed into the phase of confluent glaciation, presented a much less intelligible ground-plan pattern. In every case, however, there was still an approximately central draining canyon. It was the sprawling high-walled masses of the residual uplands that told a clear story.

The legitimate inference was that the suspension of glaciation had suspended as well a process which had threatened truncation of the range. It was obvious, postulating recession, that the canyons of this summit region were independent in their courses, and had developed independently, of the initial upland drainage plan. It was clear, from their grade profiles, that they were not stream-cut. The inference was not only legitimate, but necessitated, that, profoundly deep as they were, they were essentially of glacial excavation.

Here, then, were facts of observation in support of one of the two extreme views referred to at the outset. That, however, ice-streams had descended on the long western slope to the range foot, or even close to it, I subsequently found to be untrue.

The canyons of that flank I now regard as stream work below, as ice work above, and as the joint product of streams and of glaciers, alternately, in between. But wherever they are abnormally deep, I infer from the evidence of the floor profiles that they have been thus deepened by glaciers.

The range had not been domed over by a continuous ice-sheet; it had been glaciated rather against its upper slopes. The summit tracts, narrowed by flank attack, had remained bare, perhaps because wind-swept; the ridges and peaks of degradation continued emergent, because sharp. Obviously, though its period had been short, the action of the process had been relatively rapid; for in the shallow preglacial canyons of the broad foothill zone, in which lay the moraines of the outer glacial boundary (the relatively insignificant, coarse products of degradation), the normal processes of erosion had accomplished so little that, seemingly, their action there had been suspended.

The summation of the hypothesis was that retrogressive cutting in large part had carried away the uplands, along an approximately definite, and an approximately level, plane of attack. Deep canyons had resulted, indirectly, because recession, directed horizontally, had been directed into a rising grade. This action seemed distinct from

that of abrasion. Abrasion accomplishes deepening vertically and directly. In the case of a "continental" glacier upon a level plain, abrasion would be operative alone. But that process was not to be invoked in explanation of the scalloped, tabular forms of the High Sierra; these pointed only to basal sapping.

Basal sapping in its details, however, was unintelligible. It was not immediately apparent, at least, how a glacier, originating against a precipitous rock slope, and drawing away from that slope, could undercut and cause it to recede.

To return to the narrative form, one feature of the small ice-body lying deep in a great amphitheater opening northward from Mount Lyell—one of perhaps a dozen "glacierets" of the High Sierra—seemed to offer the explanation.

Among the numerous crevasses or schrunds of several diverse systems sharply lining the snowy surface upon which I looked directly down as upon a map, one master opening, the *Bergschrund* of the Swiss mountaineers, paralleled the amphitheater wall, a little out upon the ice. In detail it was ragged and splintered, but its general effect was that of a symmetrical great arc. I had already in mind, vaguely formulated, the working hypothesis that the glacier makes the amphitheater; that it is not by accident that glaciated mountains, and such mountains only, abound in forms peculiarly favorable to heavy snow-drift accumulation. My instant surmise, therefore, was that this curving great schrund penetrated to the foot of the wall, or precipitous rock slope, and that a causal relation determined the coincidence in position of the line of deep crevassing and the line of the assumed basal undercutting.

So much of assumption, so plausibly grounded, rendered direct observation at this critical point on the glacier floor compellingly desirable; and, returning to camp for all hands and the pack ropes, the rather appalling task was in fact very easily accomplished.

The depth of descent was about one hundred and fifty feet. In the last twenty or thirty feet, rock replaced ice in the up-canyon wall. The schrund opened to the cliff foot. I cannot say that the floor there was of sound rock, or that it was level; but there was a floor to stand upon, and not a steeply inclined talus. It was somewhat cumbered with blocks, both of ice and of rock; and I was at the disad-

vantage, for close observation, of having to clamber over these, with a candle, in a dripping rain, but there seemed to be definitely presented a line of glacier base, removed from five to ten feet from the foot of what was here a literally vertical cliff.

The glacier side of the crevasse presented the more clearly defined wall. The rock face, though hard and undecayed, was much riven, its fracture planes outlining sharply angular masses in all stages of displacement and dislodgment. Several blocks were tipped forward and rested against the opposite wall of ice; others, quite removed across the gap, were incorporated in the glacier mass at its base. Icicles of great size, and stalagmitic masses, were abundant; the fallen blocks in large part were ice-sheeted; and open seams in the cliff face held films of this clear ice. Melting was everywhere in progress, and the films or thin plates in the seams were easily removable.

These thinning plates, especially, were demonstrative of alternate freezings and thawings, in short-time intervals, probably diurnal. Without, upon the cirque or amphitheater wall, above the glacier, such intervals would be seasonal. Thus, apparently, to generalize from observation at a single point, the arc of the bergschrund foot, and the coincident arc of the cirque-wall foot, is a narrow zone of relatively vigorous frost-weathering. The glacier is a cover, protective of the rock surface beneath it against changes of temperature. Probably the bed temperature does not fall below that of melting ice. Hence, if (in summer) the bed at the wall foot is exposed, through the open bergschrund, to daily temperature changes across the freezing-point, frost-weathering must be sharply localized. The glacier will be efficient as the agent for débris removal; the result, therefore, must be quarrying and excavation, and basal sapping.

The amphitheater floor has been described as characteristically reversed in grade, though at a slight angle, and ponded backward against its head wall. It may be assumed that the disrupting action of frost at the bergschrund foot is directed against the floor, as well as against the cliff. Likely enough, also, the glacier is a highly efficient agent for removal, supplementing, by "plucking," the initial rupturing work of frost. Its plucking action may be directed downward as well as backward; but downward action, at an early

stage, will be defeated by the rapidly increasing difficulty of waste removal. The great arc-form crevasse at the glacier head, therefore, may be the indirect cause, not merely of recession of the canyon head at a low grade into a high grade, but of recession at a grade declining from the horizontal. Apparently there is a limit to such extension—a limit the earlier reached, the sharper the acclivity of the upland surface into which channeling is extended; and the head wall, in consequence, breaks back into steps, successively shortening in length of tread. The rearward steps may continue to be marked by schrunds rising to the glacier surface; living glaciers, in fact, are often characterized by "cascades" in their upper courses; but sharply defined cross-cliffs, in empty glacial canyons, are rarely to be found far down-stream. Presumably they are so deeply buried there as to be wholly left to the dulling influence of scour. The "rock-basin" lakes of the bare floors toward the head have received more attention than the great bowl of the amphitheater itself, the most phenomenal of all constantly recurring mountain forms; but I suspect that they are even more significant than has been supposed.

The hypothesis as to the action at the bottom of the bergschrund, in explanation of sapping, is slenderly supported. Physiographic inquiry has been an avocation merely, and I have failed of opportunity to repeat and to extend my early observations. Those observations, furthermore, were hastily and somewhat carelessly made, and were not recorded at the time. But that deep basal sapping, in massive rocks especially, as in the summit region of the High Sierra, accounts for the anomalies of towering upland remnants, of canyons gradeless for miles, and of the sharp scalloping in ground-plan everywhere to be observed, is at once apparent, I think, upon full recognition of the forms themselves.

With these destructional effects assigned to glacial agency, a novel possibility is at once suggested as to the part played in their persistent development by glacial scour, or coarse abrasion. The upright element in the profiles, it would seem, must be regarded as a sapping effect in which scour plays no part at all. But the approximately horizontal element, considering its great extension, often, and the relatively abrupt descent by which compensation is made, constitutes a difficulty no less. The adjusted grade in river erosion is

a smooth curve, lessening in declivity in the direction of flow. The glacier, however, by ablation, is diminished in volume as it lengthens; it is normally deepest close to its head; and possibly it is most effective in scour-erosion in proportion as it is deep. It must, in that event, tend to produce a valley "down at the heel."

The reverse grade, on amphitheater floors especially, occurs with sufficient frequency to be regarded as a type form. Rock-basin lakes, beginning at the amphitheater head, sometimes have notable length, several times the canyon width. The upper surface of the glacier here, on the other hand, invariably declines forward. Thus, in specific instances, it is not merely inference, but fact, that the glacier is deepest at the rear, and excavates there to a forward-rising grade.

It is, furthermore, implied that forward inclination of bed is not essential to glacier movement. It is not necessary, merely to determine that question, to inquire intimately into the nature of glacial motion. Fundamental in that motion, apparently, is the weight of the ice; and if the glacier at bottom, under its own weight, is not strictly viscous, it is apparently at least viscoid, responding in effect to the law of liquid pressures.

A viscous substance, heaped upon a level surface, spreads in mounded disk form, deepest at the center. Its flow-curve, in any radial vertical plane, advances from the bottom. The tendency to flow movement is proportioned to depth—to load; it diminishes toward the outer margin. The outer portions, therefore, move too slowly, and are affected by horizontal, forward thrust. They are retarded at the same time by basal friction, and in consequence present a bulged and swelling front, implying, over a broad marginal tract, rising lines of flow. But the glacier is terminated forward, and is thinned toward its termination, by combined melting and evaporation—*i. e.*, by ablation; and, by ablation, it may be inferred, the constantly bulging front is planed away. The glacier may be regarded as made up of two layers—a superficial, relatively rigid layer, and a basal layer, mobile under the weight of the other; or of a zone of fracture and a zone of flow. In the thinning frontal region, the upper layer, or cover, is brought into contact with the bed. Rearward, it is lifted; though at the same time there it is planed away. Hence, rising lines of flow in effect extend to the surface; for the cover is to

be regarded as a zone of rigidity merely, constant only as to position, and thickening, from the mobile ice below, as it is thinned by ablation above. Rates of glacial motion, measured along the surface, therefore will be deceptive. On these assumptions, the line of most rapid advance in the glacier mass is from near the bed, at the rear, to the surface, near the front. Along the bed, motion slows forward; and as pressure upon the bed diminishes in that direction, presumably abrasive erosion is most vigorous toward the rear. The accepted view as to the flow-curve of the river is that, normally, it advances most rapidly at the surface. Deep rivers, however, are found to advance from a point measurably below the surface. If rivers had the great depth of glacial streams, possibly it would appear that the curve of flow which they actually have is but the reverse curve due to bed friction, extended to the surface because the surface is near. It would seem to be a safe assertion that descending grade of bed is not essential to river motion, only decline of the river surface toward the level of discharge; and that, in a long canyon with level floor, terminating at the sea, a river, one or two thousand feet in depth and maintained at that depth at its head, would advance with essentially the same flow-curve as that here attributed to the glacier. The value of such speculation consists in the indication it affords that appeal to the observed flow-curve of the river, in rebuttal, may not be valid.

The long ribbon meadow of the lower canyon course, no less than the ponded amphitheater floor, I think, invites interpretation as the manifestation of a tendency on the part of the glacier to channel excessively up-stream. And in this overdeepening toward the canyon head, I suspect, the two agencies of horizontal sapping and of vertical corrosion powerfully co-operate.

The ultimate effect, upon a range of high-altitude glaciation, would be rude truncation. The crest would be channeled away, down to what might be termed the base-level of glacial generation. Where, among the determining causes of glaciation, high latitude rather than high altitude is operative, the base-level of degradation may lie below the sea, deepest centrally and shallowing outward. Given a land area initially, the glacier itself, as degradation approached its maximum, would replace the land, affording the

necessary above-sea surface for snow accumulation. The degradation limit would be determined by the lifting power of the sea.

The hypothesis, at this stage, is of much less importance than recognition of the anomalies of fact, of which it offers a tentative, even venturesome, explanation. In the fiorded regions of the globe, notably in the Patagonian Andes, of which a well-controlled reconnaissance survey has recently been completed, we have examples not only of fiords deepening backward for many miles into rising grades, but of fiord lakes, in parallel series, penetrating from foothills on the one side to foothills on the other, transecting a range. In explanation of such deep channels, whether occupied by arms of the sea, by lakes, or by feebly moving streams on meander bottoms, the appeal to grades, it seems to me, will be most cogent.

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SYSTEMATIC ASYMMETRY OF CREST LINES IN THE HIGH SIERRA OF CALIFORNIA.[1]

THE substance of the present paper was communicated to the Section of Physiography of the Congress of Arts and Science at St. Louis last September. The section had just listened to Mr. Johnson's paper on "The Profile of Maturity in Alpine Glacial Erosion" (this JOURNAL, pp. —). Mr. Johnson stated that since his first observations in the Sierra in 1883 his ideas as to the explanation of the phenomena had undergone development, and he regretted that he had been unable to revisit the region for purposes of verification. It was therefore a matter of gratification that I was able to supplement his presentation by the statement that during two seasons of exploration in the glaciated district of the Sierra I had found his hypothesis of cirque development by glacial sapping of the utmost utility in the explanation of the topography. It happened also that its utility was illustrated in my discussion of the origin of the special features to which my own communication referred.

In the higher part of the Sierra Nevada the glacial cirque is a conspicuous feature of the topography. Each main crest of the great mountain mass, as a rule, is bordered on each side by a row of cirques facing outward. Separating the cirques on the same side of the main ridge are subordinate ridges or spurs. Gradually the cirques unite to form glacial troughs, and these troughs are separated, at a somewhat lower level, by ridges constituting subordinate features of the range. Some of the ridges between cirques and between troughs are equally steep on both sides of their crest lines, but many —a large minority—are notably steeper on one side than on the other, and this asymmetry of cross profile is definitely related to the cardinal points. Ridges trending east and west are steeper on the north side than on the south, those trending north and south are steeper on the east side, and those trending northwest and southeast are steeper on the northeast side. In general, the gentler slope has the grade of a steep roof, and it is often clothed by rock fragments approximately

[1] Published by permission of the director of the U. S. Geological Survey.

in situ. Ordinarily it is too steep for the horse, but is readily scaled by the mountaineer. As a rule, the steeper slope either is constituted by, or else includes, an abrupt cliff which at most points cannot be climbed. Fig. 1 shows a group of high ridges in which the steeper faces are turned to the north, and Fig. 2 a group in which they are turned to the northeast.

FIG. 1.—Eastward from Mount Gardiner, Sierra Nevada. Compare the southward (right) slopes with the north-facing cliffs. [Photograph by J. N. Le Conte.]

These slopes are not controlled by rock structure. The principal rock is granite, and this granite is in large part structureless. Where it is traversed by joint systems the details of sculpture are greatly influenced by the joints, but the trend and slope of the greater features are independent of the joints.

A little reflection shows that the distribution of steep slopes is correlated with the alimentation of Pleistocene glaciers. The southward slopes of the east-west ridges, because turned toward the sun, lost more snow by melting and evaporation than did the northward slopes, and a smaller fraction of the snowfall they received remained

to nourish glaciers. Thus the glaciers resting against the southward slopes were comparatively ill fed, and the glaciers of the northward slopes were comparatively well fed. The north-south ridges may be assumed to have been swept, then as now, by dominant westerly winds, which carried much snow from the westward slopes over the crests to the eastward slopes, where it accumulated; and thus the glaciers resting against the eastward slopes were better nourished than those of the westward. These peculiarities of snow distribution

FIG. 2.—Southeastward from Alta Meadows, Sierra Nevada. Compare the northeast-facing walls of the high glaciated valleys with their southwest-facing walls.

may be readily observed at the present time. If one stands, late in summer, upon a peak in the midst of the glaciated district and looks toward the east or north, he sees bare rock, with only here and there a small remnant of snow; but if he turns toward the west or south, he looks upon a patchwork of snow and rock in which the predominance of the rock may not at once be apparent. Fig. 7 illustrates the relations of surviving snowbanks in early summer to northward and southward slopes.

It can hardly be doubted that the distribution of Pleistocene snow deposition stands in causal relation to the distribution of asymmetry in the crest lines of the minor ridges. And it is in explanation

of this relation that I avail myself of Johnson's hypothesis. Each glacier which receives more snow on one side than on the other adjusts its cross profile to a condition of equilibrium by moving away from the region of greater supply to the region of lesser supply. This lateral motion is, of course, combined with the general, and more rapid, forward motion of the glacier, but it is nevertheless competent to produce at the side of the glacier phenomena quite similar to those at the head. Fig. 3 shows diagrammatically the ground plan of a glacier flowing southward. The greatest snow accumulation is in the cirque, *AA*, where precipitation is at a maximum, where depletion through solar influence is at a minimum, and where circling cliffs protect against removal by the wind. There is great accumulation also along the west margin, *BB*, where the snow drifted by the westerly winds comes to rest in the shelter of the west wall of the glacier trough. Along the eastern border, *CC*, the snow deposit is comparatively small, because of exposure to the westerly winds. The resulting lines of ice-flow are as drawn. Moving directly away from the walls of the cirque, the glacier makes and annually renews the bergschrund, *ab*. Moving obliquely away from the west wall of the trough, the glacier similarly produces the minor bergschrund *bc*, and this minor bergschrund leads to sapping and the production of a cliff, just as the major bergschrund causes the cirque cliff. Thus the west wall of the trough is kept steep, and is thereby contrasted not only with the east wall of the same trough, but with the east wall of the adjacent trough, so that the rock crest between the two troughs is not symmetric.

Usually in viewing a cirque it is possible to trace about its wall a somewhat definite line separating a cliff or steeper slope above from a gentler, usually scalable, slope below. This line I conceive to mark the base of the bergscrhund at a late stage in the excavation of the cirque basin. I have called it in my notes "the schrund line." It can usually be traced for some little distance beyond the cirque, and sometimes for several miles on one wall or other of the glacier trough. Advancing along the trough wall, it descends gradually with a slope which may be assumed to represent the gradient of the ice surface, that surface having been somewhat higher than the schrund line. Its expression outside a cirque may be seen in Figs. 2, 5, and 6.

At a somewhat lower level than that to which the preceding para-

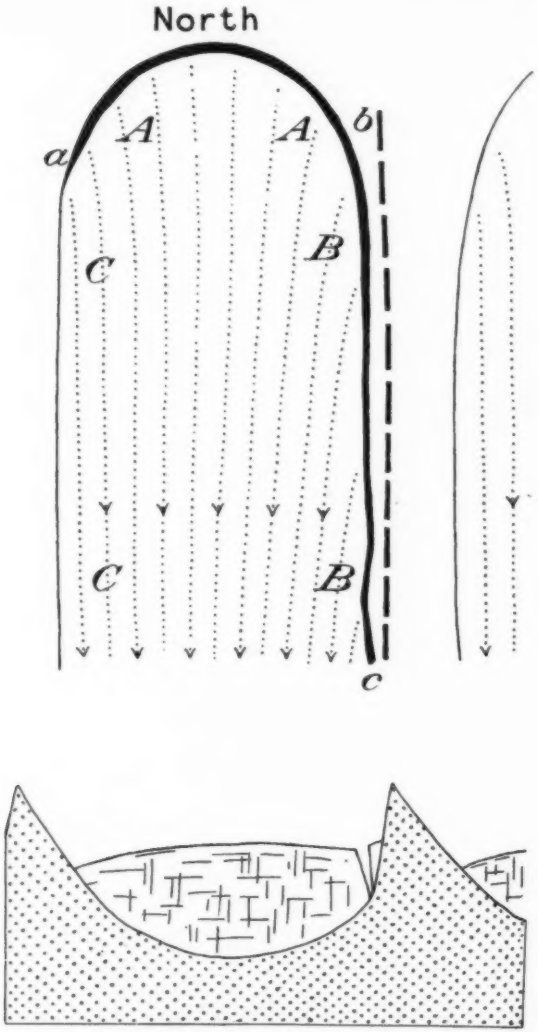

FIG. 3.—Diagrammatic ground plan and section of a glacier. The heavy line *abc* marks the position of the bergschrund. Dotted lines show direction of ice flow. The broken line marks the crest of the spur separating the glacier from its neighbor on the west.

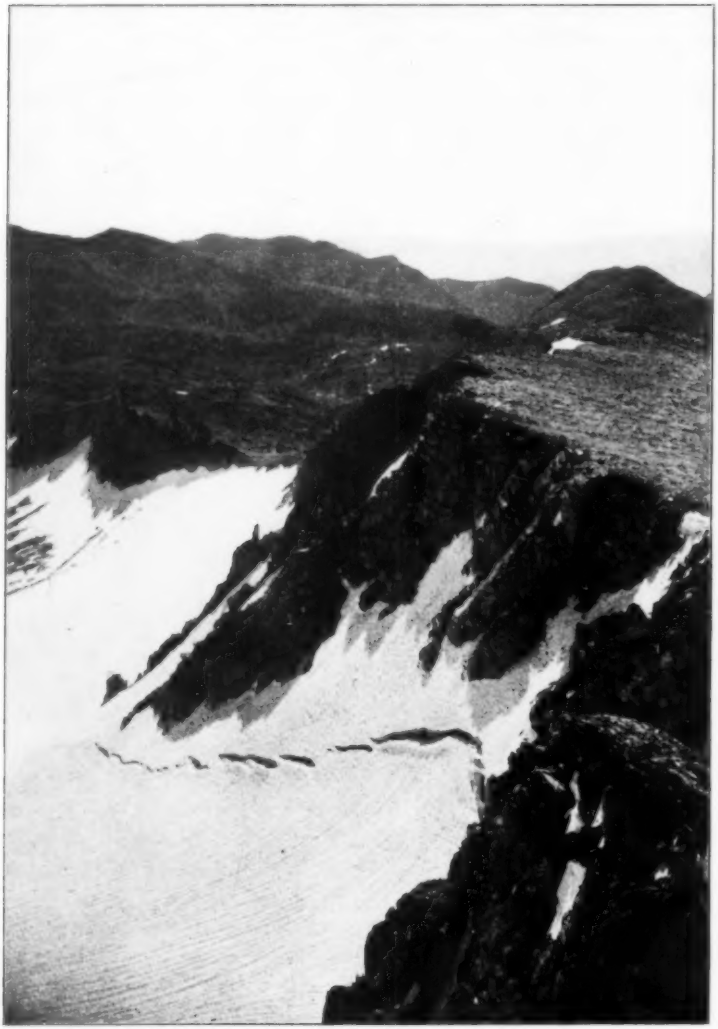

FIG. 4.—Eastward from Mount Conness, Sierra Nevada. In the foreground is the upper edge of a small glacier with its head cliff and bergschrund. Farther away the descending continuation of the same cliff springs from a schrund line, the exact position of which is masked by a lingering snow-drift.

graphs apply, the Pleistocene glaciers occupied a smaller share of the surface, and there are considerable unglaciated areas. The photograph shown in Fig. 6 was made in this region. The view is westward up the trough of a glacier. Beyond the head of this trough is another glacier trough descending westward, and the dividing ridge was partly destroyed by the head-erosion of the glaciers, so that the typical amphitheater is not shown. The south wall of the

FIG. 5.—Spur between two glacial amphitheaters near Mount McClure, Sierra Nevada, showing schrund line.

trough is steep, and shows distinct evidence of sapping. The cliff at top, being composed of thoroughly jointed granite, does not stand vertical, and the fragments recently fallen from it have built a talus which conceals the schrund line. But it is evident that here the sapping action at the schrund line was more active than the glacial erosion lower down on the slope, so as to create a sort of shoulder or terrace near that level. Stating the interpretation in another way, the excessive alimentation along the south wall of the glacier was here developing a branch glacier and a tributary cirque. This cirque was eating its way back into the ridge bounding the glacier.

On the opposite side of the ridge are gentle slopes, in part of preglacial origin. Perhaps all the portions visible are of that character, but on that side of the ridge are also faintly developed cirques, from which small ice-streams flowed toward the south.

At still lower levels are many ridges along which Pleistocene

FIG. 6.—Westward from Kid Peak, Sierra Nevada. The highest point of the crest at left is Goat Peak.

glaciers were developed on one side only—the north or northeast side, so far as observed. The south and southwest slopes retain the preglacial facies, and retain also the actual preglacial topography, except for such equable reduction of surface as may have been accomplished by aqueous and atmospheric agencies. The direction of ice movement in such cases was not parallel to the ridge axis, but approximately normal to it. The glacial excavation did not always take the character of a series of cirques, but sometimes produced a

continuous cliff, running with moderate undulation parallel to the ridge axis.

I have no photograph representing this topographic type in its purity. Fig. 7, exhibiting a ridge of greater altitude, serves to show a somewhat similar cliff, wrought by glacial head-erosion but imperfectly divided into cirques, and culminating in a crest from which nonglacial profiles descend in the opposite direction. But the example

FIG. 7.—Westward from Mount Hoffmann, Sierra Nevada. Compare the glacial topography of the north (right) face of ridge, with the nonglacial profiles toward the south. [Photograph by A. C. Lawson.]

differs from the type in the stronger expression of the ice-work, in the comparatively high grade of the nonglacial profiles, and in the fact that those profiles, as seen in the photograph, conceal south-facing cirques of some magnitude.

The asymmetry of these lower ridges is more pronounced than that of any others, because instead of contrasting two phases of glacial erosion they contrast glacial with nonglacial. It is worthy of note also, though not strictly germane to my subject, that the contrast in sculpture of the two ridge slopes serves to compare the efficiency of subaërial degradation with that of one phase of glacial

degradation. The glaciers of these low ridges, being able to develop only on the slopes most favorable for snow accumulation, marked the lower limit of névé conditions and were the feeblest of all the Sierra glaciers. Their lives must have been short, for they could exist only when glacial conditions were at or near a maximum; they began long after, and ceased long before, the glaciers of the higher districts. The topographic features they produced were subject to

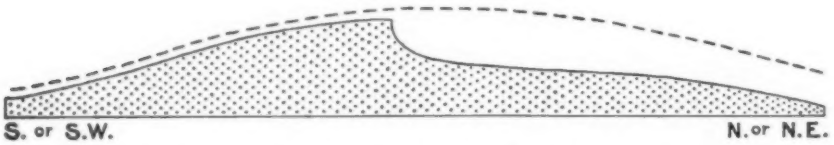

FIG. 8.—Diagrammatic cross-section of a ridge glaciated on one side only, with hypothetic profile (broken line) of preglacial surface.

the dulling influence of atmospheric and aqueous attack during both interglacial and postglacial times. And yet the degradation they accomplished was far greater than that of nonglacial agents working on the opposite sides of the same ridges—agents working not only during the same time, but during all interglacial epochs and during postglacial time. It is true that we cannot measure the nonglacial work, which consisted of a general reduction of surface without notable change of form; but whatever the amount, we may assume that it would have been the same on both slopes of the same ridge, had there been no glaciers. The visible ice-made hollows therefore represent the local excess of glacial over nonglacial degradation.

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THE PROBLEMS OF GEOLOGY.[1]

THE subject "The Problems of Geology" was assigned to me. I should not have ventured to select so formidable a topic for a brief address.

RELATIONS OF THE SCIENCES.

We are all aware that geology is a many-sided subject. While at the outset it was a simple observational study, it soon developed physical, chemical, astronomical, and biological sides. The importance of these different sides has continuously increased, so that we now often speak of physical geology, chemical geology, astronomical geology, and biological geology.

To appreciate the position of geology among the sciences it is necessary to go back to fundamental definitions. Natural philosophy in the old and broad sense may be defined as the science which treats of energy and matter. But investigations have shown that the ether also must be considered, and hence this definition needs modification. Some physicists have been inclined to extend the scope of the term "matter" to include matter in the old sense and also ether. But it seems to me that until the two, which appear to be so different, are shown to be essentially one, it is better to use the term "matter" strictly in its old sense. But it is advisable to have a term which shall include both matter and ether, and for this place the word "substance" seems suitable.[2] Using the term in this sense, natural philosophy may be defined as the science which treats of energy and substance.

Physics is the science which treats primarily of energy; chemistry is the science which treats primarily of matter. Thus physics considers mainly the actions and transformations of energy through

[1] The principal address given in the Department of Geology of the International Congress of Arts and Science at St. Louis, 1904.

[2] This definition of the word "substance" is different from that of Holman, who, as I understand it, makes the term so comprehensive as to include matter, ether, and energy. By him the word "matter" is apparently used to comprise what is here covered by both matter and ether.—SILAS W. HOLMAN, *Matter, Energy, Force and Work*, pp. 135 ff.

matter and ether; and chemistry considers mainly the actions and transformations of matter through energy. But since energy is manifest to the senses only through matter, and since matter does not exist without manifestations of energy, the relations of the two sciences are very intimate. In any book upon either subject the treatment constantly passes over to the other; indeed, energy and matter are inseparable—one cannot be considered without the other. Recently the relations between physics and chemistry have become even closer by the rise of the intermediate science, physical chemistry. This science completely bridges the gap between the two and unites them as a whole into the conjoint science of physics-chemistry, which is the science of energy and substance. As thus defined, physics-chemistry becomes a synonym of natural philosophy in its broad sense.

While physics and chemistry are really a single science, it is to be repeated that the chief point of view of physics proper is that of energy, and the chief point of view of chemistry proper is that of matter. This will be appreciated if one but mention the subjects considered in text-books of physics and chemistry. Some of the subjects of physics are sound, heat, light, and electricity. These are all forms of energy. The chief subjects for consideration by chemistry are the elements and their combinations, such as helium, chlorine, iron, calcium carbonate, etc. These are all forms of matter. Since physics-chemistry treats of all the energy and substance within the reach of our senses, physics and chemistry are the two sciences the principles of which are believed to be applicable to the entire visible universe.

Astronomy treats of energy and substance in the heavens. It is concerned primarily with the nature and development of the heavenly systems. Under the above definition, astronomy is the science of the physics and chemistry of the heavens. Biology treats of energy and substance in living organisms. Under this definition, biology is the science of the physics and chemistry of organisms. Geology treats of the energy and substance of the earth. Under this definition, geology is the science of the physics and chemistry of the earth. It includes mineralogy. These definitions may not be complete, but at least they are true so far as they go.

It is not necessary for present purposes, to consider the possible defects of the definitions given, except that for geology. Objections may be raised to defining geology as the science of the physics and chemistry of the earth, on the ground that this definition is inadequate to cover descriptive and historical geology. It may be said that it is a part of geology to describe the facts exhibited by the earth as they appear, without reference to physics or chemistry. It may be said that the history of events, as shown by the rocks and fossils, does not necessarily require physical or chemical treatment. There is some truth in these statements, but on the other side it may be held that the facts are the results accomplished by physical and chemical work. These facts become important and significant mainly as they are interpreted in physical and chemical terms. The objects of the earth—the complex results of chemical and physical work—if described without reference to the manner in which the results came about, have comparatively little interest. In reference to historical geology it may be said that this subject gives a chronological arrangement of the results of chemical and physical work.

It thus appears that physics and chemistry are the elementary sciences, while astronomy, biology, and geology may be defined, possibly with some lack of completeness, as the applications of the principles of physics and chemistry to various complex systems. In this sense astronomy, biology, and geology are applied sciences.

We are now in a position clearly to indicate the relations of geology to the sciences mentioned. So far as the earth is one member of one of the heavenly systems, it is a subject of astronomy. So far as organisms constitute a small part of the earth, they are the subject of geology. Since the earth is one of the subjects of astronomy, and since the entire kingdom of organisms constitutes a small part of the material of the earth, geology is closely related on one side to astronomy, upon the other side to biology. Geology is one of the children of astronomy. Geology begins with the earth at the time of its astronomic birth. As geology is one of the children of astronomy, so biology is one of the children of geology. As the result of various processes upon the earth, chemical and physical, organisms have been formed, and have gone through their long and complex development. But astronomy, geology, and biology—grandparent, parent,

and child—have long existed side by side, and their interaction and mutual effects have been most profound. One cannot be comprehended independently of the others.

While geology is very closely related to astronomy and biology, we have seen that it is still more closely related to physics and chemistry. Since physics-chemistry is the science of energy and substance in general, and since geology is the science of the energy and substance of the earth, geology is not simply related to those subjects—it rests upon them as its one secure foundation. They are the elementary sciences upon which geology is based; for they are the sciences of all energy and substance of which the object of geological science is an insignificant fraction.

We have now reached the most fundamental problem of geology—the reduction of the science to order under the principles of physics and chemistry. To a less extent geology is subject to the sciences of astronomy and biology.[1]

While the relations of geology to the other sciences, as above set forth, are incontestible, it was possible to appreciate those relations only after the sciences were well developed. Geology did not begin consciously as the science of the physics and chemistry of the earth. The phenomena of the earth were studied as objects, and thus geology

[1] The earth is the vastest aggregate of matter within the direct reach of man. By a study of a small part of this aggregate the principles of physics and chemistry have been formulated. The material which has been studied is but an inappreciable part of the material of the earth, and but an infinitesimal part of the substance of the universe. Yet the doctrine is unhesitatingly accepted that the principles of physics and chemistry, wrought out with reference to this minute fraction of substance, are not only applicable to all the materials of the earth, but to all parts of the visible universe. This daring generalization has received astonishing confirmation by studies of other portions of the visible universe through the spectroscope and photographic plate.

In the generalization that the principles of physics and chemistry, developed by study of small masses of material, apply to all parts of the universe, we have a case of the extension of a generalization from a part to the whole, which surpasses almost any similar extension of reasoning. Indeed, some philosophers have seriously questioned the legitimacy of the conclusion.

In view of the foregoing, it is rather curious that the geologist now finds his most important problem, the problem of problems, in the explanation of phenomena exhibited by the heterogeneous earth in terms of those principles of physics and chemistry built up mainly by observation, experiment, and reasoning upon a minute fraction of the earth.

was at first an observational study. The next step, a revolutionary one, was to explain the observed phenomena in terms of physical and chemical processes, many of which could be observed. But few have asked the question: "What is a geological process?"

GEOLOGICAL PROCESSES.

It is a curious fact that, while the word "process" is used in innumerable geological papers and text-books, I have been unable to find anywhere a definition of a "geological process."

I shall define a "geological process" as the action of an agent by the exertion of force involving the expenditure of energy upon some portion of the substance of the earth.

Physical definitions of "force," "work," "energy," and "agent."—In order to understand the above definition of "geological process" it is necessary to define the terms "force," "work," "energy," and "agent."

Hoskins defines "force" as action exerted by one body upon another tending to change the state of motion of the body acted upon.[1] According to Daniell's more simple definition, "force" is any cause of motion.[2]

When a force applied to a body moves the body in the direction toward which the force acts, it does work.[3] In this sense "work" is the product of force into displacement, the common formula being $W=FS$. The unit of work is defined as the quantity of work done by a unit force acting through a unit distance.[4]

Hoskins defines "energy" in the terms of force and work. Thus he says when the condition of a body is such that it can do work against a force or forces that may be applied to it the body is said to possess energy. The unit of energy is the same as that of work.[5] According to Daniell's more simple definition, "energy" is the power of doing work.[6]

The order of definition of the above terms is that in which knowledge of them has developed. The actions of forces in doing work are

[1] T. M. HOSKINS, *Theoretical Mechanics*, 1900, pp. 2 and 16.

[2] ALFRED DANIELL, *A Text-book of the Principles of Physics*, 3d ed. (1895), p. 4.

[3] HOSKINS, *op. cit.*, p. 298.

[4] *Ibid.*, p. 298.

[5] *Ibid.*, p. 308.

[6] DANIELL, *op. cit.*, p. 2.

observed. From such observations the existence of energy is inferred. Wherever forces act upon matter and work is done, energy must exist. Further reasoning shows us that bodies may possess energy which is latent and is not exerting force. Hence many physicists have defined "energy" without introducing the words "force" or "work." Thus, according to Holman, "energy" is power to change the state of motion of a body.[1] If energy be recognized as the primary thing, then "force" can be defined in terms of energy. According to Holman, "force" is that action of energy by which it produces a tendency to change the state of motion of bodies.[2] Similarly, the word "energy" may be introduced into the definition of the word work. Thus Holman says "work" is that action of energy by which it produces motion in a free body, or produces or maintains the motion of a body against resisting forces.[3]

An "agent" is any portion of the substance of the earth which may exert force and thus expend energy to perform geological work. Thus ether, air, water, and rock are agents.

The next step in the comprehension of geological processes is a consideration of the kinds of energies, forces, and agents, and their relations.

Kinds of energy and force.—Ultimately the forms of energy may be reduced to a few, and possibly to a single kind. Indeed, some physicists believe that all forms of energy are really but different manifestations of kinetic energy. But the number of elementary kinds of energy in the universe is a problem for the physical philosopher, not the geologist. The geologist is concerned in all the kinds of energy which he observes at work. These are: (1) gravitation energy, (2) heat, (3) elasticity energy, (4) cohesion energy, (5) chemical energy, (6) electrical energy, (7) magnetic energy, (8) radiant energy (including radiant heat, radiant light, and electro-magnetic radiation).[4]

From another point of view energy may be classified into kinetic energy and potential energy. Under static conditions of all the parts of a system any or all of the kinds of energy above named may be exerting force, but so long as no motion occurs and no work is done they are all potential. When anywhere in the system move-

[1] SILAS W. HOLMAN, *Matter, Energy, Force and Work*, 1898, p. 20.

[2] *Ibid.*, p. 41. [3] *Ibid.*, p. 17. [4] *Ibid.*, p 37.

ment takes place and work is done, some portion of the energy becomes kinetic. Work and kinetic energy are inseparable. As multifarious kinds of work are always going on in the world, potential and kinetic energy are always existent. For the most part we can trace the kinetic energy back to one or more of the various classes of energy above mentioned, but some part of it may be derived from other unnamed sources.

Any of the forms of energy may exert force, hence we have the terms "force of gravitation," force of heat," "force of elasticity," "force of cohesion," "chemical force," "electrical force," "magnetic force," and "radiant force."

Any or all of these forces may be exerted both under static and dynamic conditions. When the conditions are static, the energy is potential. When the conditions are dynamic and work is done, some portion of the energy is kinetic. To illustrate: For many years a cliff may stand; but finally a portion of it falls and geological work is done. The force of gravitation is exerting the same pressure upon the material concerned during all the years of quiesence and during the brief period of movement, and for that matter continues to be exerted after movement ceases. During the static conditions the energy of gravitation is potential. During movement some part of it, by pressure of the force of gravitation, passes into kinetic energy. And this energy, through the agency of the falling part, the agent, does further geological work upon the material at the foot of the cliff.

All of the forms of energy and force are important in geology, but the geological work of some of them has been more clearly discriminated than that of others. For instance, the geological results produced by electricity and magnetism have not been worked out, although I have no doubt that electrical and magnetic energy have produced important permanent effects upon the earth which ultimately will be discriminated.

Geological use of the words "force," "energy," and "work."— To the present time the geologist has much more frequently used the word "force" than "energy." This is because the geologist is usually more concerned with the exertion of force by an agent than he is with the source or amount of energy which the agent contains. Physical investigations seem to show that substance contains enor-

mous quantities of energy, only a small part of which is manifest to the senses, and this only under special circumstances. So far as geological bodies have great stores of energy which are not manifest as force, there is no change of condition—no geological process. The geologist is primarily concerned with the energy which is manifesting itself either statically or dynamically by the exertion of force. Consequently, he more often refers to the forces of geology than the energies of geology. This is the more natural since the unit of force and the unit of energy are the same, and that energy is measured only by its action as a force. While in the past the primary interest of the geologist has been in force rather than in energy, it is probable that in the future he will become more and more concerned in the energy itself and its sources.

Often the geologist has made no discrimination between the words "force" and "energy." He has frequently used "force" in the old sense, both to cover the thing itself, the energy, and the action of energy, the force, in accomplishing work. This formerly was the practice of physicists also, who, for instance, spoke both of the conservation of force and the exertion of force. If the conclusion be correct that the source and amount of energy concerned in a process should be discriminated from its action as a force, it is clear that the time has now come when the geologist must in his writing clearly differentiate the two ideas.

Since the physicist now makes an important discrimination between the words "energy" and "force," it may be necessary for the geologist to follow him in his definitions of these words, although much can be said against technicalizing and narrowing the use of the general term "force." Probably the interests of all the sciences would have been best subserved if the physicists had introduced a new word for the technical sense assigned to the word "force," and had left this term to be used in the general way in which it has been used in the past in science, and will continue indefinitely in the future, to be used in literature. This is especially true since, if we confine the word "force" to its physical definition, we are in constant need of a word to cover both energy and force, as defined by the physicists. If the latter word be technicalized, I can think of no better word than "power" for the conception which includes both. This

was the word used for this place by Hutton in the opening pages of his epoch-making paper on the "Theory of the Earth."[1]

It is also to be noted that the word "work," as above defined, is also technicalized, having reference only to the exertion of force in producing change of state of motion. With this meaning it has no relation to the material results. To illustrate: By the expenditure of energy, the crust of the earth may be fractured, or material be transported from one place to another. In the general sense used by geologists, these results are often spoken of as "work." It is certainly a very grave question whether geologists can afford to restrict the word "work" to its physical definition, and thus be obliged to discontinue its use in an indefinite sense, both for the expenditure of the energy, and the effect of such expenditure, or for either alone. While this is so, it may be said there are very considerable advantages in having a technical word for the physical meaning of work. This would assist the geologist to think clearly and discriminate between the expenditure of energy and the material effects of such expenditure.

Whatever meaning the geologist assigns to the words "force" and "work," he should have a clear understanding of the conceptions which the physicists have of their meaning, and should attempt to express these conceptions in some way. Also he should make it clear, in case he decides not to use the words "force" and "work" in the physical sense, that the old general usage is retained for them. In this paper I shall use "force" in its technical sense, but retain the common usage for the word "work."

The agents of geology.—We are now ready to classify the agents of geology. They may be grouped into ether, gases, liquids, and solids. Possibly organisms are so peculiar a combination of gases, liquids, and solids that they should constitute a fifth group, and in this case the agents may be classified into ether, gases, liquids, solids, and organisms. From another point of view the agents may be classified into their chemical elements, some seventy or more in number, but of which only about twenty are so abundant as to be important.

The small number of categories of energies and agents given

[1] CHARLES HUTTON, "Theory of the Earth," *Philosophical Transactions of the Royal Society of Edinburgh*, 1785, pp. 212–14.

might lead to the conclusion that the subject of geology is reduced to simpler terms than is really the fact. Each of the forms of energy, gravitation, heat, elasticity, cohesion, chemical affinity, electricity, magnetism, and radiation is most complex and acts as forces in most diverse ways. The number of gases, of liquids, and of solids which occur in nature are beyond number. They are most diverse in character. For instance, the liquids vary from nearly pure water to magma. The solids comprise all kinds of minerals, of which there are many hundreds, and the various combinations of these minerals in rocks, the different phases of which are very numerous. Gas without the presence of liquids and solids, liquids without the inclusion of gases and solids, and solids which contain no gases or liquids, while perhaps possible in a physical or chemical laboratory, are not found in nature. As remarked by Powell, gases, liquids, and solids are everywhere commingled upon the earth. All are commingled with ether. Thus the various combinations of agents are beyond computation. Also definite agents, for instance water may occur in various kinds of bodies, each of which acts in a manner peculiar to itself.

The materials upon which the agents act are of the same kinds, and have the same diversities and complexities, as the agents themselves. Moreover, the work done inevitably affects both the material acted upon and the agent. The agent that grinds the rock-floor at the bottom of a glacier is also ground. This necessity of work upon both agent and substance acted upon comes under the law of Newton in reference to action and reaction. The fact of work both upon agent and substance upon which the agent acts raises the question as to the distinction between the two. The answer is: The agent is the substance containing energy which it expends in doing work upon other substances. The substance upon which work is done may thereby receive energy, and thus become an agent which does work upon other substances; and so on indefinitely. Indeed, the rule is that one process follows another in the sequence of events, until the energy concerned becomes so dispersed as to be no longer traceable. Theoretically this goes on indefinitely.

Analysis of geological processes.—We have seen that the action of one or more agents through the exertion of force and the expenditure

of energy upon one or more substances is a geological process. It is rare indeed, if it ever happens, that a single agent works through a single force upon a single substance. Commonly two or more agents are doing work by the expenditure of energy of various kinds at the same time upon more than one material. The processes of geology, therefore, vary in their complexity from the action of a single agent through a single force upon a single substance, to the action of all kinds of agents through all classes of force upon the most diverse combinations of substances. Thus the solution by rain-water of pure calcite is a process. Also erosion, which is the work of all the agents by the expenditure of various kinds of energy upon the most diverse combinations of materials, is called a process. It is plain that the number of processes of geology, comprising as they do all possible combinations of energies, agents, and substances, are beyond number, if indeed they are not infinite. If geology is to be simplified, the processes must be analyzed and classified in terms of energies, agents, and results. Each of the classes of energy and agent should be taken up, and the different kinds of work done by it discussed. For instance, the work of the force of gravitation through gases, liquids, and solids should be analyzed. To some extent this has been attempted, but very imperfectly indeed. And such discussion has scarcely been seriously undertaken for the other forms of energy. Text-books should consider each of the classes of energy by itself, the nature of the forces it exerts, the processes through which it works, and the results accomplished through the various kinds of agents.

The general work of each of the agents and the results accomplished should be similarly considered. Not only so, but the work of the different forms that each of the agents takes should be separately treated. Thus, besides considering the work of water generally, the work which it does both running and standing must be treated. The first involves the work of streams; the second, the work of lakes and oceans. This involves the treatment of streams as entities, or, to use a figure of Chamberlin's, as "organisms." The treatment of the work of gases should involve the subjects of gases of the atmosphere, gases of the hydrosphere, and gases of the lithosphere. The treatment of the agents will be more satisfactory in proportion as the work done by each of the forms of each of the agents

is explained under physical and chemical principles in the terms of energy.

It is plain that the treatment of the energies of geology and the treatment of the agencies of geology will overlap, since one cannot be considered without also considering the other; but this is an advantage rather than a disadvantage, for each of the two points of view is very important in enabling the mind to grasp the composite whole. Just as in the science of physics-chemistry it may sometimes be advantageous to consider the subject mainly from the point of view of substance, and at another time mainly from the point of view of energy, and the treatments from both points of view are necessary to build up the science of physics-chemistry; so it is necessary to consider the subject of geology from the points of view of energy and of agent, if an approximation to adequate comprehension be gained.

As already intimated, another point of view from which geology may be considered is the result. This was the chief point of view of the early geological papers and text-books, which were content to tell of phenomena. Phenomena may, and often are, observed and described in advance of their physical-chemical interpretation. But the naming or even the description of the phenomena of the earth without reference to energy or agent is very unsatisfactory. And usually the valuable descriptions of before unobserved phenomena are made in connection with theories of their physical and chemical significance. But it is still true that observation and description present a third important point of view which interlocks with and overlaps the treatment of geology from the points of view of energy and agent.

So complex is the earth that to enable the mind to comprehend the intricately interlocking whole, the subject should be considered from as many points of view as possible. If only the human mind were sufficiently powerful, and means of expression adequate, the ideal method of treatment would be simultaneous consideration and exposition of all possible points of view. But since this method of treatment is an impossibility, we must necessarily at any time consider each portion of the subject in part and treat it in part. The problem is then the selection of the various partial points of view

which are important, and the determination of the order of their consideration.

No one, I think, can hold that any of the points of view above mentioned—process, energy, agent, and result—is unimportant in a general treatment of the subject of geology. It is therefore clear that all these points of view must be handled. There may be difference of opinion as to the order in which they shall be presented; and for different parts of the subject of geology and for different purposes the best order will vary.

We are now in a position to foresee the future development of the science of geology. The early papers and text-books were content to tell of accomplished results. Almost nothing was said with reference to processes. As the science developed, there crept into the literature of the subject more and more reference to processes. The present year a text-book of geology by Chamberlin and Salisbury has appeared, the first which avowedly attempts to treat geology from the point of view of processes rather than phenomena.[1] This is a great step in advance. But a large part of the task of reducing the processes to order in terms of energies, agents, and results still remains to be done. When this is accomplished, we shall have a statement of the principles of geology in terms of physics and chemistry.

How knowledge of processes has developed.—The principles of geology have been developed in the past and will continue to be developed in the future both from the study of processes now in operation and by the consideration of the results of processes which cannot be observed. An excellent illustration of a branch of geology, the principles of which have largely been established by the observation of processes now in operation, is furnished by physiography. So far as one can see, the surface of the land is now being modified by the energies and agents of geology as rapidly as at any time in the past. These energies and agents may have varied in their efficiency from time to time and place to place, but the above statement is broadly true. There are other branches of geology, the principles of which have been mainly developed from results accomplished rather than from observation of the present actions of energies and agents.

[1] CHAMBERLIN AND SALISBURY, *Geology*, Vol. I, "Processes and Their Results," 1904.

In such branches the probable energies, agents, and processes which produced the observed results were developed from a consideration of the methods by which chemical and physical energy through the agents available could have produced the results observed. For instance, the development of the solar system occurred but once. During that development the earth was formed, including the atmosphere, hydrosphere, and lithosphere. The process of differentiation was not observed by man, cannot be repeated by him. The only method of reaching a probable conclusion as to the manner of accomplishment of the complex result is to consider in what possible ways physical and chemical energy may have acted upon the enormous masses of universe stuff out of which the earth was constructed, and to check this reasoning by the attainable knowledge of what is now occurring upon other heavenly bodies.

The qualitative and quantitative stages of explanation.—The task of explaining geology in terms of processes involving energy and agent has two stages—the qualitative stage and the quantitative stage. For most problems we have as yet been unable to go beyond the qualitative stage. In the qualitative stage of a problem it is shown that a cause is real. In this stage the question is not asked as to how far the explanation applies; *i. e.*, its quantitative importance. Most geologists are content when they reach the qualitative stage. A certain cause is determined to be real in the explanation of certain phenomena. It is then usually assumed that this cause is the only cause. For instance, it has been generally accepted that the loss of heat by the earth results in decreased volume, and that such condensation is a cause for crustal deformation. Many geologists have stopped at this point satisfied. They have not asked the question: To what extent can loss of heat by the earth explain crustal deformation, and are there any other causes which can be assigned? Some years ago I listed a number of causes, each of which partly explains deformation. In addition to secular cooling, they are as follows: volcanism, cementation, change of oblateness of the earth, change of pressure within the earth, change of physical condition of the material of the earth, and loss of water and gas from the interior.[1]

[1] C. R. VAN HISE, "Estimates and Causes of Crustal Shortening," JOURNAL OF GEOLOGY, Vol. VI (1898), pp. 10–64.

Evidently, in order that we may have even an approximately correct idea of the chief causes for crustal deformation, the question must be answered as to the quantitative importance of each of the causes.

The consideration of the processes of geology by quantitative methods is superlatively difficult, yet this task must be undertaken if the science ever approximates certainty of conclusions. This leads to the relations of mathematics to geology. The moment we pass to the quantitative treatment of processes the assistance of mathematics is needed. For simple quantitative calculations arithmetic and algebra may suffice, but for the more difficult problems of geology the assistance of higher mathematics is needed. This, then, raises the question as to whether or not it is expected that the geologist, in addition to knowing physics and chemistry, must also be a mathematician. Undoubtedly this is the ideal equipment of a geologist, which, unfortunately, few if any po.. ess. There are many geologists who apply simple mathematics to various problems. But the man who is so familiar with forces, agents, processes, and phenomena of geology that he is able to handle them, and at the same time is capable of handling higher mathematical reasoning, is rare indeed. Those geologists who have made the attempt to combine mathematical with their geological reasoning usually have shown marked deficiency in their mathematics. Upon the other hand, those mathematicians who have attempted to handle the problems of geology mathematically have usually been so deficient in a knowledge of geology that their work has been of comparatively little value. In view of these unfortunate results, it seems to me that the time has come for co-operation between geologists and mathematicians in the advancement of the science of geology to a quantitative basis. Two or more men should work together, some of them geologists with a broad familiarity with the phenomena and methods of their science, and the others expert mathematicians. In continual consultation, the geologist and mathematician will be able safely to handle the problems of geology quantitatively. This happy condition of co-operation, once reached, will be sure rapidly to advance the science.

The quantitative solution of geological problems is likely to emphasize also another of the principles of geological method of the greatest

importance. The causes offered to explain the phenomena do not exclude one another. It is believed that each of them is a real cause, and partly explains the phenomena—that the different causes are complementary. While a majority of geologists have been content with suggesting a single physical cause for a phenomenon, others have taken more than one possible cause into account. Thus Chamberlin[1] has formally adopted the method of multiple hypotheses. But the great majority of those who have considered more than one hypothesis in connection with a geological problem have carried on their discussions as if one of the suggested causes must be selected to the exclusion of the others.

As a matter of fact, almost every complex geological phenomenon has not a simple, but a composite, explanation. To illustrate, in Chamberlin and Salisbury's text-book of geology it is stated that the explanation of volcanism may be given upon the assumption that the lavas are original; or, second, on the assumption that the lavas are secondary. Under the first assumption it is suggested (1) that lava outflows from a molten interior, and (2) that lavas flow from molten reservoirs. Under the second assumption it is suggested that lavas may be assigned (3) to the reaction of water and air penetrating to hot rocks, (4) to relief of pressure, (5) to melting or crushing, (6) to melting by depression, and (7) to the outflow of deep-seated heat.[2] At the close of the discussion it is said that these hypotheses "must be left to work out their own destiny."[3] I fear many will make the inference, although I have no idea that the authors so intended, that one among these hypotheses will be victorious in the struggle for existence and the others totally overthrown. My point in this connection is that the two main suppositions, and all of the hypotheses under them, may be true in part; that these various explanations are not necessarily exclusive of one another, but may be supplementary. When we have a quantitative discussion of the probable effects which may be expected from each

[1] T. C. CHAMBERLIN, "The Method of Multiple Working Hypotheses," JOURNAL OF GEOLOGY, Vol. V (1897), pp. 837–48.

[2] CHAMBERLIN AND SALISBURY, *Geology*, Vol. I, "Processes and Their Results," 1904, pp. 595–602.

[3] *Ibid.*, p. 602.

of the causes suggested, we shall have some idea of their possible relative importance. For my own part I have no doubt whatever that volcanism is to be explained by some combination of the seven causes mentioned, with doubtless other causes which have not yet been suggested, rather than by a single cause. As soon as it is appreciated that to explain a complex phenomenon several causes are usual, if not invariable, rather than exceptional, it becomes plain that their relative importance should be determined, and this can be done only by quantitative methods.

THE INDIVIDUAL PROBLEMS OF GEOLOGY.

Thus far we have been considering the problem of geology as a general one. The subject assigned, "The Problems of Geology," might imply a treatment of the particular problems at present being considered by geologists. For an address this interpretation of the subject is impracticable. Adequately to discuss one of the unsolved problems of geology from the point of view advocated would require a monograph. Not only is it impossible to discuss unsolved problems of geology, but it is impracticable, within the limits of this paper, even to list the problems demanding solution. As evidence of the correctness of this statement it may be noted that a subcommittee of the Carnegie Institution stated scores of problems upon the investigation of rocks, the statement of which, limited to the briefest possible terms, occupies a number of printed pages.[1]

ILLUSTRATIONS OF TREATMENT OF GEOLOGICAL PROBLEMS FROM THE POINT OF VIEW OF ENERGY, AGENT, AND PROCESS.

While it is not practicable to discuss, or even to list, the particular problems of geology, it is possible to mention illustrations of the systematization and simplification of the science by the treatment of processes in terms of energy and agent. These I shall take from my own publications, for the reason that I can more easily give them than any others. My chief subjects of study have been (1) the gross and minor deformations of the lithosphere, and (2) the interior transformations of the rocks, or metamorphism. When I began the study of the first of these subjects, I found a heterogeneous mass of facts in reference to the deformation of many regions, with various guesses

[1] *Carnegie Institution Year-Book*, No. 2, pp. 195–201.

as to how the results came about, but with no consistent attempt to reduce the many observed phenomena to order under the principles of physics and chemistry. The subject of metamorphism was in an even worse condition. The work upon this subject was of the most random character; indeed, nothing short of chaos prevailed. A person who attempted to carry the multitudinous statements of facts in his mind would need more than cyclopedic powers of memory, and the statements would not even have had the artificial order of an encyclopedia. I became convinced that, if the treatment of metamorphism was to continue along the old lines, the subject was doomed to hopeless confusion.

With the above condition of affairs before me, I set about attempting to ascertain the principles which control the various kinds of deformation of rock masses, and which underlay the transformation of rocks. It soon became plain to me that the task was a great problem in applied physics and chemistry. When this was realized, it became clear that it was necessary to know the principles of physics applicable to the deformation of matter and to the alteration of rocks. Thus my first task was to remedy the defects of my basal training by gaining a working knowledge of the well-established principles of these subjects. This task I found a formidable one, which occupied much of my time for several years, and which I can claim to have only very imperfectly accomplished.

In order to understand the diverse phenomena of crustal deformation, it was plainly necessary to know the principles of deformation of small masses, such as can be handled in the laboratory. Unfortunately it was found that this part of the subject of physics is in a very imperfect condition. No systematic statement is available as to the manner in which different substances behave under varying conditions of stress. While studies have been made of the deformation of iron under a moderate range of conditions, comparatively little has been done concerning brittle bodies such as constitute the rocks. Exact knowledge is needed as to the behavior of rocks under the most extreme variations of stress, temperature, amount of water, and other conditions. But while it is highly desirable to have this knowledge, the geologist cannot wait until it is available. The only practicable course is to study closely the phenomena of rock deforma-

tion, and interpret these facts in the light of the physical and chemical knowledge available.

A broad study of the phenomena of deformation by various men showed two classes of very diverse phenomena. In some areas the prominent deformations of the rocks are those of fractures, such as joints, faults, brecciations, etc. In other places the deformations are mainly those of flexure. For instance, in some places one finds that brittle rocks, such as jaspilite and quartzite, are deformed almost wholly by numerous fractures, and in other places have been bent within their own radius, or even minutely and extremely crenulated with no sign of fracture. A close study of the geological conditions under which these two classes of deformation occurred shows that the more modern rocks, which have at no time been very deeply buried, are those which are most likely to exhibit only the effects of rupture; whereas the ancient rocks, and especially those which have been deeply buried, are likely to show the evidence of profound folding without rupture, although often there is superimposed upon the flexures more recent fracture deformation. Physical experiments had shown that, when a brittle substance like a rock is stressed beyond the limit of elasticity under the conditions of the earth's surface, that cohesion is overcome, and rupture takes place. This fact, correlated with the general observation of rupture in recent rocks and those deformed near the surface, led to the conclusion that normally the deformation of the outer part of the earth is by fracture.

After this conclusion was reached, it was a natural step to the conclusion that at a very moderate depth below the surface of the earth the superincumbent pressure is greater than the strength of any rock, and that, if openings could be supposed to exist, they would be closed by pressure; in other words, that the pressure due to the force of gravitation is sufficiently great so that the molecules of the rocks are held within the limits of molecular attraction or are within the limits of the force of cohesion. This naturally led to the suggestion of a deep-seated zone of rock-flowage, in opposition to a zone near the surface, that of fracture. At the time this conclusion was reached, no experiments had been made actually showing the deformation of rocks under the conditions of the deep-seated zone, but since that time Adams and Nicolson have deformed

rocks by flowage in the laboratory.[1] Thus observation of the geologist, inference from the observation, and experimental work have led to advance in the science of physics.

For the present purpose the important thing is to observe that a realization of the very diverse results which follow from deformation under different physical conditions led to a satisfactory classification of two great sets of phenomena which had been noted, but without any reason being assigned why one occurs at one place and the second at another place. Thus in the text-books of geology joints, faults, and folds were described. But there was no attempt to explain why fracture occurred here, folds there, and in a third place both. After it was realized that the great earth-movement makes joints, faults, and other fractures at and near the surface, and at depth, below these structures, other structures which have been called folds, it was possible to reduce the gross deformation of rocks to some systematic order under the principles of physics. There of course remains the working out of the precise physical conditions which result in the various diverse phenomena. For instance, what are the exact conditions of stress which result in the many complex systems of joints? While progress has been made upon this and other problems of gross deformation, a vast amount of work remains to be done before the subject will be even approximately reduced to order in the terms of energy, agent, and process.

It has already been intimated that the subject of rock alteration was in an even more unsatisfactory state than that of gross deformation. The particular alteration of this or that rock was given without any adequate consideration of the geological, physical, or chemical conditions under which the change took place. Thus there were many thousands of descriptions of rock alterations, but no understanding of the reasons why the particular alteration for a given rock found at a given place occurred. To make the matter worse, almost every description of rock alteration was accompanied by vague guesses as to the causes of the changes, the majority of which were little short of grotesque.

[1] F. D. Adams and J. T. Nicolson, "An Experimental Investigation into the Flow of Marble," *Philosophical Transactions of the Royal Society of London*, Series A, Vol. CXCV (1901), pp. 363–401.

After it was appreciated that the gross deformation of rocks is very different in an upper and a deeper zone, the question naturally arose as to whether there are not differences in the rock alterations in these zones. This idea, when followed up, resulted in astonishingly fruitful results. It was found that in the upper zone, that of fracture, the chief alterations which take place are those of oxidation, carbonation, and hydration. These reactions occur with liberation of heat and expansion of volume. In other words, the reactions are controlled by chemical energy. In the lower zone the dominant factor controlling alterations is physical energy. Pressure diminishes the volume. In order to accomplish this, the chemical reactions of the upper zone are reversed. Deoxidation, silication with decarbonation, and dehydration occur with absorption of heat. The reactions controlled by the force of gravitation are under the principles of physics. It thus appears that the reactions of the two zones are opposed throughout. It is plain that if the subject of metamorphism is to be reduced to order, the alteration of the upper zone, that of fracture, must be discriminated from that of the deep-seated zone, that of rock-flowage.[1]

The working out of the principles of metamorphism was a physical-chemical problem. The handling of the problems of rock alteration with fairly satisfactory results was possible because of the rise of physical-chemistry. Had this science not been developed within the past score of years, it would not have been possible to have gone far upon the problem of metamorphism.

It is to be emphasized that gross deformation is not independent of metamorphism, or metamorphism independent of gross deformation; the two interlock. The general solution of the problem of gross

[1] The necessarily narrow limits of this paper render it extremely difficult to show the manner in which the subject of metamorphism has been treated under the system advocated as a general method for geology. By referring to *Monograph XLVII of the United States Geological Survey*, a treatise on metamorphism now just appearing, the reader will better appreciate the illustration. In this volume the forces of metamorphism, the agents of metamorphism, and the zones of metamorphism, are first fully treated, the point of view being mainly physical-chemical. After the general principles contained in these chapters are given, the alterations in each of the different belts and zones are developed. The point of view of the latter chapters is mainly geological, but the geology is interpreted in the terms of the principles earlier formulated.

deformation made it possible to formulate the principles controlling the interior transformations of rocks. In a similar manner these problems interlock with the other problems of physical geology, and physical geology interlocks with the other sides of the subject. The whole science is one interlocking system, a part of which cannot be satisfactorily developed independently of the other parts. For instance, weathering can be placed in order only when considered in connection with general metamorphism, erosion, and sedimentation. Ore deposits can be explained only by combining the principles of volcanism, deformation, metamorphism, etc.

In attempting to reduce a small part of the subject of geology to order under the principles of physics and chemistry, the plan was followed of oscillating between observations of the facts as exhibited in the field and laboratory, and their physical-chemical explanation. After a large number of facts were observed in the light of known principles, the attempt was made better to formulate the principles which explain them. After this was done, the facts were again more comprehensively studied in the field and in the laboratory in the light of the new principles. The statement of principles was then modified and improved by use of the new facts. The improved statement of principles was again tested by further facts. Thus the process of development has been a series of approximations toward both completeness of statement of fact and perfection of formulation of principle, but neither have been attained, nor, so far as we can see, will they ever be reached.

NECESSITY FOR ADVANCE IN THE SCIENCES OF PHYSICS AND CHEMISTRY.

Very often, in the attempt to find principles applicable to the phenomena of deformation and metamorphism, it has been found that the science of physics-chemistry is not sufficiently advanced to make this possible. In such cases physicists and chemists have been asked to develop this subject at the needed points. But at innumerble places the problems have proved to be so numerous and complex that the necessary aid has not been obtainable. Thus there has arisen, with reference to my own work, a great line of unsolved problems which demand the co-operation of physicists and chemists. The same is true of the work of all other geologists interested in the

fundamental problems of geology. As a consequence, when a committee was appointed by the Carnegie Institution to consider what could best be done for the advancement of geology, it was unanimously decided that the most pressing need of the science was, not further support of the study of the phenomena of geology, but the advancement of the principles of physics and chemistry upon which geology is based.[1] In a small way some of the physical and chemical problems, the solution of which are asked by geologists, have been taken up by the Carnegie Institution. Thus the demands of the geologists that their science shall be reduced to order under the principles of physics and chemistry are likely to result in important advances of these sciences.

DEFECTS OF GEOLOGICAL LITERATURE.

If further proof than that already given were needed of the importance of the knowledge by geologists of the basal principles of the elementary sciences, and of their application to geological problems, it is furnished by the literature of geology. It seems to me that the radical defect which pervades the literature of the subject is due to the lack by geologists of such knowledge. Because of this, many geologists are wholly unable to make a logical arrangement of their material, or respectably to discuss the phenomena observed with reference to causes.

Indeed, some geologists seem to take pride in lack of knowledge of principles and of their failure to explain the facts observed in the terms of the elementary sciences. I have heard a man say: "I observe the facts as I find them, unprejudiced by any theory." I regard this statement as not only condemning the work of the man, but the position as an impossible one. No man has ever stated more than a small part of the facts with reference to any area. The geologist must select the facts which he regards of sufficient note to record and describe. But such selection implies theories of their importance and significance. In a given case the problem is therefore reduced to selecting the facts for record, with a broad and deep comprehension of the principles involved, a definite understanding of the rules of the game, an appreciation of what is probable

[1] *Carnegie Institution Year-Book*, No. 1, 1902, No. 2, 1903.

and what is not probable; or else making mere random observations. All agree that the latter alternative is worse than useless, and therefore the only training which can make a geologist safe, even in his observations, is to equip him with such a knowledge of the principles concerned as will make his observations of value.

It is doubtful if more than one or two text-books of geology have been written which do not contain many statements capable of arousing the amusement of the physicist. When the geologists who write the standard books of the science are properly equipped with a working knowledge of the principles of physics and chemistry, the books will cease to be a heterogeneous mass of undigested material mingled with inferences as to the meaning of the phenomena, which to anyone familiar with the principles of physics and chemistry are often ludicrous. From the above point of view, it might be said that the problem of geology, the problem of problems, is to get men who write geological papers and books so well trained in the elements of the sciences upon which geology is based that they shall be able to reason correctly as to physical and chemical causes, and consequently to observe and describe accurately and discriminatingly. It is plain that the geologist who hopes to advance the principles of his science must have a working knowledge of physics and chemistry.[1]

PRINCIPLES OF GEOLOGY THE SAME FOR THE ENTIRE EARTH.

The phenomena of geology for any extensive area—for instance, a continent—are so numerous that, had the science originated in Europe, in America, and in Asia independently, the principles of the science developed in these three regions would have been essentially the same. The chief differences would have been that the emphasis placed upon the different principles would have varied. Also the principles of certain divisions of the subject would have been somewhat more fully developed in one case than in another. For instance, because of differences in the range of latitude and other climatic conditions, certain parts of the principles of physiography would have been more fully developed on one continent than in another.

It is, of course, understood that the foregoing statements premise

[1] C. R. VAN HISE, "Training and Work of a Geologist," *Proceedings of the American Academy of Sciences*, Vol. LI (1902), pp. 399–420.

that men of equal ability and attainments had been at work on the problems of geology in the various continents. This supposition is, of course, erroneous, for it is evident that the great constructive work of geology has been done largely by a comparatively few individuals. Indeed, the contrast between nations in the number of creative geologists which they have produced is so great that it is a fair inference that the differences in the principles of the science developed in the three continents under the conditions named would have been more largely due to difference in the capacity of the geologists than to variation in the phenomena demanding explanation. In geology, as in other lines of human endeavor, the exceptional man, the genius, is a factor of paramount importance.

THE PROBLEMS OF PROVINCES AND DISTRICTS.

Thus far we have considered only the development of the principles of geology. They are applicable to the entire earth. There is another great field of geology, which has not yet been suggested—the application of the principles to provinces and districts.

This second line of problems of geology is illustrated by such subjects as the stratigraphy of a given district, its physiography, its paleontology, etc. The working out of the stratigraphy, or physiography, of a given county or township may be of great importance to the inhabitants of that county or township, or even of some consequence to the nation. They are, however, of much less importance to persons interested in the advancement of the principles of geology, unless their elucidation adds to the science some new principle, or some unusually fine illustration of an old principle.

The principles of geology may be broadly comprehended by a single individual. No individual can be familiar with more than a minute fraction of the applications of the principles to the numerous geological provinces of the world. Scarcely a score of years ago it was possible for a geologist not only to know the developed principles of the science, but to know somewhat fully the facts upon which those principles were based. At the present time this is impossible. A man may know the more important facts in reference to a few districts, the broader facts in reference to states, and some of the more general facts in reference to an entire continent, or even more than

one continent; but no man can know more than an inappreciable portion of the geological facts of even the countries which have been somewhat closely studied; and these countries comprise but a small part of the earth.

But it is unnecessary for a man to know all the facts of geology. He need only know the more important facts for a sufficiently broad region so that he may understand the recognized principles of the science, assist in their development, and take part in the discovery of new principles. The discoveries will be found to be largely applicable to the vastly greater regions of the world which are not considered by the discoverer. All this is very fortunate for the science of geology. A student beginning the subject may fully comprehend the truthfulness of many principles which have been developed in various parts of the world through the illustrations furnished by his native parish.

From the foregoing it appears that the geology of the future is to have two aspects, which, as time goes on, will become more and more clearly differentiated: first, the principles of geology; second, the application of principles to various parts of the world.

CONCLUSION.

It is clear that the evolution of the science of geology has followed a strictly natural course. Before the subject was recognized as a science, the earth was being observed. When man turned to nature-study, he began to observe the phenomena exhibited by the earth, such as the stratification of the rocks, and the presence in them of objects which are called fossils. After such observations were made, it was inevitable that sooner or later the question should arise as to the manner in which the results observed were accomplished. Thus the observation of phenomena led to a study of processes. Sands like those observed in a consolidated form were seen in the process of building. The conclusion followed that the consolidated stratified rocks were formed by the processes observed upon the seashore. Sea shells were seen to be produced by animals and to be deposited with the upbuilding sands. This led to the explanation that the fossils in the sedimentary rocks were due to the processes observed.

After a large number of explanations, the methods of which were the same as in the illustrations given, the general doctrine was evolved

that the geological results of the past are to be explained by present processes, or the present is the key to the past. While the above conclusions now seem almost axiomatic, we need not go far back to find them astonishing novelties. So far as we are aware, the natural explanation of fossils was first reached by that amazingly versatile genius, Leonardo da Vinci, in the fifteenth century. The conclusion that the present is the key to the past required for its formulation the intellect of the great Hutton.[1] It was not announced until 1785, and the doctrine was not generally accepted until after Lyell's *Principles* appeared in 1830.

As the science of geology developed, the practice of explaining the phenomena in terms of processes gradually became more common, until, as we have seen, it is dominant in the latest geological text-book. But, as already intimated, the analysis of processes in terms of energy, force, and agent has only begun. It is my belief that at some time in the future a text-book of geology will appear which begins with a discussion of the energies, forces, and agents of geology, the understanding of which is necessary in order adequately to comprehend processes. It has been stated that the problem of geology is the reduction of the science to order under the principles of physics and chemistry. This is equivalent to saying that the problem of geology is the discussion of the subject in terms of energies, forces, agents, processes, and results. Such a discussion will constitute the principles of geology.

It is my deep-seated conviction that by the solution of this problem only can geology be so simplified as to be comprehended with reasonable fulness by the human mind. When this work is done, the broad principles of the science will be capable of statement with

[1] How clearly the great Hutton appreciated the doctrine commonly called that of uniformity is shown by the following quotations from his "Theory of the Earth": "In what follows, therefore, we are to examine the construction of the present earth, in order to understand the natural operations of time past; to acquire principles, by which we may conclude with regard to the future course of things, or judge of those operations, by which a world, so wisely ordered goes into decay; and to learn by what means such a decayed world may be renovated, or the waste of habitable land upon the globe repaired." The concluding sentence of his work is: "The result, therefore, of our present inquiry is, that we find no vestige of a beginning—no prospect of an end."—CHARLES HUTTON, "Theory of the Earth," *Philosophical Transactions of the Royal Society of Edinburgh*, 1785, p. 218; *ibid.*, p. 304.

comparative simplicity and brevity. But so broad and complex is the science of geology that a comprehensive statement of the principles of the entire subject will necessarily be somewhat voluminous.

Supplementary to the principles of geology, which are applicable to the entire earth, there will be a long series of volumes of the geology of different continents, the various political divisions of these continents, the states under those divisions, or even the minor areas, such as counties or townships; for so numerous are the facts of the science that it requires a volume to discuss in detail even a small area. For instance, to give the geology of a township with sufficient fulness to make clear the earth-story there illustrated may require a good-sized volume.

We have seen that geology rests upon physics and chemistry as its foundation; that it is closely related upon one side to astronomy, upon another side to botany; that in its broader sense it includes mineralogy; and that for its satisfactory development the aid of the higher mathematics is needed. It is evident that the man who is to advance geology must be broadly trained in science, and that he have a firm grip upon the nature of energy, ether, and matter, and their interactions.

It is my conviction that when geology is placed in order under the principles of physics and chemistry the science will have passed through a greater revolution than at any previous time in its history.

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GLACIAL AND POST-GLACIAL HISTORY OF THE HUDSON AND CHAMPLAIN VALLEYS. II.[1]

OUTLINE—*Concluded.*

HISTORY.

- Hudson water body and successive positions of ice-edge as it retreated through Hudson Valley.
- Successive positions of the ice-edge in the passages from Hudson to Champlain Valley.
- Successive positions of ice-edge in Champlain Valley.
- Hudson-Champlain water body.
- Higher Glacial Lake Champlain.
  - Erosion of Fort Edward Valley—making upper series of terraces.
- Lake St. Lawrence.
- Marine Champlain.
  - Making lower series of terraces.
  - Extended course of Poultney-Mettawee River.
- Inauguration of present Lake Champlain.
  - Drowning of lower Poultney-Mettawee River Valley.
  - Cutting of outlet of Lake Champlain.
- History in Hudson Valley in Higher Glacial Lake Champlain time and later.
  - Disappearance of Hudson water body.
  - Trenching of old lake- or old sea-floor of Hudson Valley.
  - Origin of Hudson Deeps.
  - Drowning of lower Hudson.
  - Post-Hudson-Champlain changes of drainage in the Hudson Valley.
  - Piracy of Outlet Creek and beheading of Drummond Creek.
- Correlation of Terraces in the Champlain Valley with those in the Hudson Valley.
  - First assumption.
  - Second assumption.
  - Another interpretation.
- Altitude of Hudson water body.
- Origin of Hudson water body.
  - Lake hypothesis.
  - Sea hypothesis.
  - Origin of northward rise of gravel plateaus on each hypothesis for Hudson water body.
  - Origin of submerged channels on each hypothesis for Hudson water body.

[1] Continued from p. 469.

Origin of the gaps in the moraine.
Explanation of scarcity of wave-wrought features in Hudson valley under each hypothesis for Hudson water body.
Features unexplained by salt-water hypothesis of Hudson water body.
Absence of distinct wave-wrought features at outer edge of Brooklyn-Perth Amboy moraine.
Presence of overwash plains at the ice-front.
Absence of life certainly marine in Hudson water-body deposits.
Absence apparently of tidal distribution of muds in Hudson water body.
Altitude of Hudson water body.
Evidence that Hudson water body was a lake.
Arguments opposed to the Hudson lake hypothesis.
Relation of Hudson water body to Connecticut Valley water body.
Relation of Hudson water body to water body west of Palisade Ridge.
Relation of Hudson water body to Lake Iroquois.
Relation of higher glacial Lake Champlain to Lake Iroquois.
Duration of Hudson water body.
Time divisions.

HISTORY.

HUDSON WATER BODY AND SUCCESSIVE POSITIONS OF THE ICE AS IT RETREATED THROUGH THE HUDSON VALLEY.

As the ice retired from the Brooklyn-Perth Amboy moraine northward, it halted for a greater or less time at the successive positions which are marked on the higher lands by belts of thick drift, with more or less distinct morainic topography, by elongate kame areas with the aspect of moraines, often bordered by plains of gravel and sand having the form of overwash or outwash plains, or by aggradation plains without moraine or kame areas at their source. On Staten Island possibly one morainic belt of limited extent, and on Long Island at least two and probably three such morainic belts, mark some of the halting-places of the ice, north of the main moraine. On the Triassic lowlands in New Jersey, no less than seven such positions are marked by belts of thick drift with either the moraine or kame aspect. (See Fig. 8, p. 427.) In the lower ground, both in the lowland west of the Palisade Ridge and in the Hudson Valley, the ice and the ice-waters discharged into a standing body of water. In the low ground, west of the Palisade Ridge, the deposits of the ice-waters are marked by the complex series of sand- and gravel-

plains or plateaus, some of them heading in kames and others with ice-molded but kameless sources, which is found from the latitude of Hackensack and Englewood nearly to the northern border of the state. The deposits of the ice-waters are also marked by the clay which is found underneath the gravel and sand in the southern part of these lowlands, or spread out with little overlying sand or gravel, and which has thicknesses from 100 feet or less, to 215 feet.[1]

That the water body in which these deposits accumulated may have been separated from, and perhaps was slightly higher than the Hudson water body will be shown later (p. 645). It is to be noted that the accumulations marking the successive positions of the ice-edge on the higher land are not traceable across the lowlands occupied by this water body, at least not to the same extent, either in number or continuity, as on the higher land.

In the Hudson Valley the deposits marking the successive positions of the ice-edge do not have notable development south of Sing Sing, but from a little north of this place to north of Glens Falls, and beyond into the Champlain Valley, there is a succession of deposits, described above (pp. 430 ff.), which, it is believed, mark its successive positions. As the ice retreated northward, the ice-front appears to have assumed two distinct phases in different parts of the valley.

Phase 1.—In those parts of the valley (notably the narrower parts) where the gravel plateaus are marked either by morainic phenomena or by irregularities of similar import at the edge next to the Hudson, or by higher elevation next to the Hudson and lower next to the valley wall, and with layers dipping toward the valley wall and southward, it is believed that the ice protruded down the valley, and that the accumulations took place at the edge of this ice-tongue, or between

[1] See *Annual Report* of the State Geologist of New Jersey for 1903, pp. 195–210, and *Final Report*, Vol. V, pp. 506–13, 595–628, 632–42. At the time this report was written three hypotheses were suggested to explain the form of these higher gravel plains and plateaus; namely, (1) that they were accumulated in a water body, either a lake or an arm of the sea; (2) that they had received their form from stagnant ice-masses; (3) that both co-operated. In the absence of wave-wrought features, and in the absence of exposures, the junior author preferred to leave open the question of the origin of these features, where the structure was unknown, although at this time, and for some time before, it had been recognized that a water body existed in the Hudson Valley as the ice was retiring, and that both ice and water body had been influential in producing the forms there found.

the ice-tongue and the valley wall. Such deposits, it is believed, are represented (1) by the terrace south of the Croton River mouth (Fig. 9, No. 20, p. 429); (2) by the moraine on the north slope of the Palisade Ridge, which has not the accompanying gravel plateau (Fig. 9, No. 15); (3) by the Jones Point gravel plateau (Fig. 9, No. 19); (4) by Roye Hook (Fig. 9, No. 29), and possibly that part of the State Camp plateau which appears to slope eastward. In some places where tributary streams head northward the ice occupied the upper portion of the tributary valley at the same time that the ice-tongue existed in the main Hudson near its mouth, so that deposits with layers dipping toward the valley wall contributed from the ice-tongue in the Hudson Valley, occur side by side with deposits from the tributary streams of ice-water which show layers dipping toward the Hudson. The main part of the State Camp plateau, which appears to have been built of materials brought by streams of ice-water down the valley of the Peekskill and its tributaries, is thought to be an example. Other phenomena which indicate the presence of the ice in the valley, against which stratified drift was accumulating at higher levels, but to which this valley ice was not active in contributing, it is thought, may be represented by the deposits at Carthage Landing and Low Point (Fig. 9, No. 44), and at New Hamburg (Fig. 9, No. 46). At the latter place, however, the waters from the ice in the valley may have been active contributors in building the plateau, at least in its early stages. (5) The West Point gravel plateau, and probably the Cold Spring kames and the Cold Spring-Garrisons terrace (Fig. 9, Nos. 31, 33) also represent deposits made at the edge of a tongue of ice which occupied the valley.

Phase 2.—The second phase of the ice-front is represented in the broader parts of the lower Hudson and in the broad upper Hudson where the gravel plateaus are marked by moraines or kames or undulatory topography of similar import, at the margin toward the valley walls, and by the smoother surface and steep outer face toward the Hudson, together with the dip of the layers generally away from the valley wall, and the gradation of the materials down the dip from coarse gravel into sand and finally into clay. This clay spreads out in the upper Hudson as a wide plain, rising gradually from the present Hudson River bluffs toward the gravel plateaus and the valley

walls. In these parts of the Hudson it is believed that an embayment in the ice-front existed in the deeper water over the lower parts of the plain, and that the ice-edge is marked (1) by the series of gravel plateaus with the characters above mentioned at their upper margin; (2) by the kames fronted by clay-plains without intervening gravel plateaus; and (3) by the series of elongate depressions like those between the plateaus of the series south of Saratoga Springs now occupied in part by lakes, such as Round Lake and Saratoga Lake, Lonely Lake, and perhaps also Ballston Lake.[1]

Such a form of the ice-front is marked, it is believed, by the deposits (1) at Croton and Croton Landing, and at Haverstraw and North Haverstraw (Fig. 9, Nos. 22, 15, and 17); (2) at Newburg-New Windsor, and Fishkill-Dutchess Junction (Fig. 9, Nos. 37, 38, and 42, 41). Other places where the ice halted are marked (1) by the South Schodack gravel plateau (Fig. 13, No. 73, p. 436) and the line of kames extending northwest of East Greenbush (Fig. 13, No. 74), by kames near Teller Hill, and the line extending through North Albany to Newtonville (Fig. 13, No. 65), (2) by the South Bethlehem gravel plateau (Fig. 13, No. 62); (3) by kames near New Scotland and Voorheesville (Fig. 13, Nos. 63 and 64); (4) by the Troy gravel plateau and kames (Fig. 13, No. 76); (5) by the series of gravel plateaus separated by elongate depressions, south of Saratoga Springs, where several successive positions of the ice-edge are marked; (6) by the succession of kames and gravel plateaus near Glens Falls, where several positions of the ice-edge are marked (Fig. 13, No. 85, and Fig. 18, west of No. 86, and Nos. 87 to 89, p. 454). This includes the Glen Lake kame area north of Glens Falls.

The depth of water into which the ice flowed and built up kame areas and similar deposits appears in places to have been considerable, as much as 60 to 80, or possibly 100 feet, if the evidence furnished by the Teller Hill kames, southeast of Albany (elevation of top, 280 feet) and the adjacent South Schodack-East Greenbush gravel plateau (elevation, 340–360 feet) be correctly interpreted. The 260–280-foot Lonely Lake gravel plateau (Fig. 13, No. 83) was built in water

[1] The writer does not mean to imply here that the plateaus between these depressions were built only from successive positions marked by the depressions, for probably the building was in process during the retreat from one depression to the next succeeding.

which was 100 feet deep over the plateau, if the adjacent plateaus be correctly interpreted. These figures are in accord with those showing the depth of water in which the ice succeeded in making a subdued moraine in the basin of Lake Passaic. They do not show the total depth of water in the water body, but the depth only in which the ice was able to build the moraine, kames, etc., mentioned. If the proportion of ice to débris carried were known, it would furnish a means of estimating the thickness of the ice on these moraines and kames.

In the Hudson Valley no less than fifteen halting-places are thus indicated, and of these at least six are marked by distinct morainic phenomena. This does not take account of the area between Poughkeepsie and Catskill, which was observed only in transit.

SUCCESSIVE POSITIONS OF THE ICE-EDGE IN THE PASSAGES FROM HUDSON TO CHAMPLAIN VALLEY.

The successive positions of the ice as it retreated from the Hudson Valley into the Lake Champlain region are not known. In the western or Lake George passage, after having built the Glen Lake-Hopkins Pond kame area (Fig. 18, Nos. 87–89), thus forming the dam that blocks the valley and makes the basin in which southern Lake George is situated, the ice is not known to have made notable deposits until the northern end of Lake George is reached, where the western passage opens out into the Lake Champlain Valley. In the eastern passage the successive positions of the ice-edge are not known. It seems probable, however, that the ice-front had a direction such that local lakes were formed in tributary valleys in which clays similar to those of the Hudson and Champlain regions were deposited, but at levels higher than those reached by the Hudson water body.

SUCCESSIVE POSITIONS OF ICE-EDGE IN CHAMPLAIN VALLEY.

The successive positions of the ice in the Lake Champlain Valley are not well known. Some of its positions are marked by the terraces: (1) at Baldwin and northward (Fig. 18, No. 105, *A*, p. 455); (2) at Street Road (Fig. 18, No. 107) and northward; (3) by the moraine northwest of Crown Point (Fig. 18, northwest of No. 108); (4) by limited gravel areas along the mountain-side from Port Henry

to Westport; (5) by the Bouquet River high-level delta; (6) by the Reber and Towers Forge gravel plateaus or deltas on the North Branch of the Bouquet River; (7) by the moraine or series of moraines along the higher land between Harkness and Schuyler's Falls, and at Cadyville and west toward Dannemorra; (8) by the Saranac high-level gravel plateau; (9) by kames or moraine west by south of West Chazy.

HUDSON-CHAMPLAIN WATER BODY.

When the ice had retreated into the Champlain Valley, the Hudson water body occupied the lowland between the Hudson and the Lake Champlain Valley also, and may now be conveniently referred to as the Hudson-Champlain water body (see Fig. 22).[1] The successive positions of the ice as it retreated up the Champlain Valley are known to the writer at a few points only, and these are on the west side of the valley. The moraines found above the highest level reached by the water body near Crown Point, and at the various places indicated in the detailed description above (pp. 462, 463), between Harkness Station and Cadyville, and west of the latter place toward Dannemorra, all indicate a general north-south and northeast-southwest direction of the ice-front, and also indicate that the ice was in the lower land and its edge was on the slopes of the higher land. This appears likewise to have been true when the Baldwin and Street Road plateaus were built. Where the ice-edge and the body of ice were at Crown Point when the highest deposits of lacustrine origin were deposited is not known, although doubtless, it could be determined by detailed investigation. It seems difficult to reconcile the eastward dip of the stratified drift built out in the water body with the position of the body of the ice in the lowlands when the moraine higher on the mountain-side was built. The deltas, both on the main Bouquet River and on the north branch, are so situated that neither the assumption of an embayment in the ice-front nor a protruding tongue of ice down the Champlain Valley is necessary to explain the phenomena. The deposits of gravel at 580 feet on the Ausable may be interpreted

[1] This name was first given to the water body occupying the Hudson and Champlain Valleys by MR. WARREN UPHAM, who published on this subject in 1891 in the *Bulletin of the Geological Society of America*, Vol. III, pp. 484–87, and in the *American Journal of Science*, Vol. CXXIX (1895), pp. 13 ff.

FIG. 22. FIG. 23. FIG. 24.

FIG. 22.—Approximate area once covered by the Hudson-Champlain water body on the assumption that it was a lake. The outline does not include the highest levels reached as the ice was retreating from the Brooklyn-Perth Amboy moraine to Kill van Kull, nor does it include the extension of the area into Long Island Sound.

FIG. 23.—Approximate area once covered by Higher Glacial Lake Champlain. No attempt is made to show the northern limit.

FIG. 24.—Approximate area covered by "Marine" Champlain. No attempt is made to show the northern limits reached by these waters.

with either form of front. The highest Saranac gravel plateau indicates the presence of the ice at its northern margin, and possibly some of the phenomena of the western margin indicate its presence there, but this plateau is not well known. It will be referred to again.

On the whole, then, the moraines of the west side of the Champlain Valley, at high levels, indicate a retreat of the ice with a general north-south or northeast-southwest front, and with the body of the ice in the valley. At lower levels, in general, no embayment in the ice-front seems to be required by the gravel plateaus, although deposition of some of the stratified drift in the water body is difficult to understand if the ice occupied the lowlands, and there was no embayment. The high-level Saranac gravel plateau or delta is probably an example. It seems necessary to believe that when the moraines at high levels near Harkness and west of Cadyville were being built local lakes existed at the front of the ice in the valleys of streams now flowing into Lake Champlain, at levels higher than the level of the water body in the Champlain Valley.

HIGHER GLACIAL LAKE CHAMPLAIN.

As the ice retreated through the Champlain Valley, an uplift took place at the south which separated the water body in this valley from the Hudson water body south of Fort Edward and inaugurated the history of a water body which Baldwin first named Glacial Lake Champlain. In view of the fact that another glacial lake may be represented by the upper part of the lower series of terraces, it seems best to call it Higher Glacial Lake Champlain. This lake drained southward through the Fort Edward Valley and across the barrier south of Fort Edward. Whether the Hudson water body continued to exist for any length of time after the inauguration of Higher Glacial Lake Champlain is not known. Indeed, it is not known that its disappearance may not have been on the appearance of Higher Glacial Lake Champlain. By this time, or earlier, those peculiar conditions which it appears had existed through much of the history of the Hudson water body, and had prevented the making of distinct wave-wrought features, ceased to be effective, and the upper series of wave-wrought features, which may be seen from Street Road north,

was made. Contemporaneously with at least the lower terraces of this upper series, the Fort Edward outlet valley was eroded. The question as to where the ice-edge was when Higher Glacial Lake Champlain was inaugurated will be referred to presently, as will also the greater range of the upper series of wave-wrought features from Street Road to north of Crown Point. When the lowest terrace of the upper series was made, the water-level remained constant long enough for a delta to be built on a number of the northern streams. These deltas are capable of another interpretation, however, as will be seen later.

LAKE ST. LAWRENCE.

After the upper series of terraces had been completed, the Fort Edward outlet was abandoned, and the water-level fell rapidly to the upper terrace of the lower series. Whether this level was the sea-level or not seems uncertain. The level of the marine fossils falls below it 70–80 feet at the north, and not far from that amount at the south, so far as the writer has been able to discover.[1] If this water-level, represented by the upper terrace of the lower series, was not the sea-level, then it represents a lake-level made by the opening up of some outlet, presumably toward the St. Lawrence, which was lower than the Fort Edward outlet. The location of such an outlet is entirely unknown, and its existence is hypothetical. In 1895 Mr. Warren Upham suggested such a lake "occupying an area from Lake Ontario to near Quebec," and "dating from the confluence of Lakes Iroquois and Hudson-Champlain." Concerning it he says:

> From the time of union of Lakes Iroquois and Hudson-Champlain a strait at first about 150 feet deep, but later probably diminishing on account of the rise of the land about 50 feet, joined the broad exposure of water in the Ontario basin with the larger expanse in the St. Lawrence and Ottawa valleys and the basin of Lake Champlain. At the subsequent time of ingress of the sea past Quebec the level of Lake St. Lawrence fell probably 50 feet or less to the ocean level. The place of the glacial lake so far west as the Thousand Islands was then taken by the sea.

As thus defined, Lake St. Lawrence would fall in with the non-fossiliferous part of the lower series of terraces in the Champlain region. It is to be noted, however, that these terraces do not belong

[1] If fossils occur up to 250 feet, south of Vergennes, as reported by early investigators, the above does not hold.

to the Hudson-Champlain water body, nor even to the Higher Glacial Lake Champlain water body, but were made after the abandonment of the Fort Edward outlet. In 1903 Mr. Upham referred the low-level delta of the Hudson at Fort Edward and certain high-level terraces in Chesterfield in the Champlain region to Lake St. Lawrence. As will be seen from the foregoing account of the history of this region, the present writer considers this low-level delta at Fort Edward as having been made either in the latest stage of the Hudson-Champlain water body or in the earliest stage of Higher Glacial Lake Champlain, and the high-level terraces in the northern part of the Champlain region as having been made in Higher Glacial Lake Champlain. If the non-fossiliferous levels in the lower series of terraces in the Lake Champlain region be referred to Lake St. Lawrence, the Fort Edward delta and the high-level terraces in the northern Lake Champlain region cannot be so referred. Since it is doubtful if the water body in which either the high-level terraces or the Fort Edward delta were made reached to the St. Lawrence, it would seem to be inappropriate to call it Lake St. Lawrence. Altogether it seems best to reserve this name for the hypothetical lake which followed the union of the water bodies in the Ontario and Lake Champlain regions, as originally defined by Upham.

MARINE CHAMPLAIN.

If the upper terrace of the lower series represents the sea-level, then, on the abandonment of the Fort Edward outlet, the history of the Higher Glacial Lake Champlain was closed and that of Marine Champlain was inaugurated. If during the fall of Higher Glacial Lake Champlain level to the upper terrace of the lower series there was no change in the altitude of the land, then, since the difference in level between the two series is generally 120 feet, Higher Glacial Lake Champlain must have been at its closing stage 120 feet above sea-level, and at its higher stage, barring uplift during its history, it must have been at least 75–100 feet higher. If the upper terrace of the lower series of terraces does not represent the sea-level, but does represent a lake-level, then Higher Glacial Lake Champlain was more than 120 feet A. T. when its outlet was abandoned. It is to be noted that the level of the Fort Edward outlet valley at White-

hall is close to 120 feet A. T., and if Higher Glacial Lake Champlain at the close of its history was 120 feet above sea-level, then there has been no change in level in this part of the outlet since that time, but farther south, at the 160-foot divide near Fort Edward, there has been an uplift of more than 40 feet.

During Marine Champlain time the lower series of terraces was made in the Champlain region from the uppermost marine level down to near the present Lake Champlain levels. Since the uppermost terrace of the lower series, when projected southward, falls below the Fort Edward outlet level, and since marine fossils have not been found south of Port Henry, where they were found at a level of 140 feet and lower, it is believed that the sea did not reach south as far as the Hudson Valley. It has been calculated, by projecting the terrace gradient southward, that Benson Landing or Putnam Station was approximately the southern limit reached by the waters forming the upper terrace of the lower series. During the time in which the lower series of terraces was being made, which it will be convenient to refer to as "Marine" Champlain[1] time, uplift was taking place, greater at the north than at the south, thus producing a wider range of the lower series of terraces at the north than at the south.

EXTENDED COURSE OF POULTNEY-METTAWEE RIVER.

While the waters of the south end of "Marine" Champlain were receding northward on account of the uplift of the land, or at first on account of the cutting of the outlet of Lake St. Lawrence, if "Marine" Champlain includes lake terraces, and later on account of the uplift of the land, the streams which had flowed into Higher Glacial Lake Champlain, and which, because of the sudden fall of its water-level, had extended their courses across the old lake-floor plain to the new shore, finally were extending their courses across the old sea-floor to the receding shore. This was true of the Poultney and Mettawee Rivers, which during Hudson-Champlain and Higher Glacial Lake Champlain time debouched into that water body by independent mouths (see Fig. 25, *A*). On the fall of Higher Glacial Lake Champlain to "Marine" Champlain they became united and with other formerly independent

[1] "Marine" Champlain levels would thus include both those of the hypothetical Lake St. Lawrence and the levels marked by marine fossils, which are called Marine Champlain levels (without the quotation marks).

streams extended their courses across the newly exposed lake-floor from near Whitehall to the new water-level, which was, perhaps, somewhere near Putnam Station. The main stream of these united streams is referred to as the Poultney-Mettawee River (see Fig. 25, *B*). As the "marine" water body gradually withdrew, this stream extended its course to the new levels, and finally at the close of Marine Champlain time, on the inauguration of present Lake Champlain, it had its mouth some five miles northeast of Port Henry (Fig. 25, *C*, E) where, apparently, it built out a delta which is now about 23–51

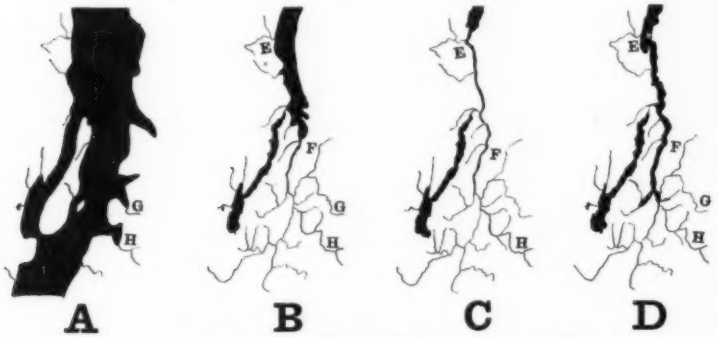

FIG. 25.—Changes in the Poultney-Mettawee River System.

A, the system dissevered in Hudson-Champlain time; *B*, the united and extended courses of the Poultney and Mettawee Rivers and their tributaries at the beginning of "Marine" Champlain time; *C*, the same at the close of "Marine" Champlain time or on the inauguration of present Lake Champlain; *D*, the Poultney-Mettawee system dissevered by the tipping of the water of Lake Champlain into the southern end of its basin caused by differential northern uplift; *E*, Port Henry; *F*, Benson Landing and Putnam Station; *G*, Poultney River; *H*, Mettawee River.

feet above sea-level or 50–78 feet below lake-level (Fig. 20, p. 467). During "Marine" Champlain time this stream cut out the channel in the clay plain described above (pp. 466–68) fromWhitehall to Benson Landing and northward to beyond Port Henry. Deposits made by the stream at successive positions of its advancing terminus, other than the submerged delta, have not been recognized, but they are in large part beneath the waters of the lake. The earlier deposits, however, should be found at low levels north of Benson Landing.

INAUGURATION OF PRESENT LAKE CHAMPLAIN.

The emergence from the sea of the barrier which makes present Lake Champlain, closed Marine Champlain history and inaugurated

present Lake Champlain. By greater northern uplift Lake Champlain has been warped into the southern part of its basin, thus submerging the lower Poultney-Mettawee Valley, dissevering its system, and drowning the lower courses of its tributaries, including the South Bay Creek, and numerous other streams in southern Champlain (see Fig. 25, *D*, Fig. 20, and Fig. 21).

The streams in northern Champlain did not have their courses extended because of the uplift, for the outlet at the north controlled the water-level. Their courses have been extended only by the lowering of the water-level because of the cutting down of the outlet—an amount which Baldwin[1] has placed at 50 feet, but concerning which the writer has made no observations. Terraces lower than those of Marine Champlain have been made in present Lake Champlain and exposed to view by the lowering of the water-level due to the cutting of the outlet.

With this uplift, greater at the north than at the south, came the revival of north-heading streams and the arrest in the development of south-heading streams—a process which seems, from the topographic maps, to be well shown by East Creek, and by Dead Creek and its south-heading tributaries. Revival seems to be shown by the north-heading tributaries of Dead Creek.

The rapid down-cutting of the Poultney-Mettawee River, because of its steep northward gradient (120 feet in 14 miles, if the estimates of elevations at the close of Marine Champlain time be correct) surpassed that of most of its tributaries, which had neither the advantage of the steep northward gradient (since most of them had either an eastward or westward flow), nor had they the advantage of the volume of water of the larger stream. Consequently these tributaries were left to descend over steep slopes into the valley of the main stream (see Fig. 16, *A*, p. 452). The larger tributaries have been able to push this steep part of their gradient farther back from the main stream than the smaller ones. South-heading tributaries, with the advantage of the steep gradient given by the attitude of the land when the Poultney-Mettawee was cutting its channel, were more successful in keeping pace with the cutting of their mains, but since the northern uplift they have been arrested in the continuance

[1] *American Geologist*, Vol. XIII (1894), p. 104.

of this work, while north-heading tributaries have been given the advantage. It is believed that on some of the streams this record can be read from the topographic maps.

HISTORY IN HUDSON VALLEY IN HIGHER GLACIAL LAKE CHAMPLAIN TIME AND LATER.

The history of the Hudson Valley has been left at the point where the southern uplift brought a barrier south of Fort Edward into effective position and inaugurated Higher Glacial Lake Champlain (see Fig. 23). How long the Hudson water body survived is not known. It is not known, indeed, that its history overlaps to any extent the history of Higher Glacial Lake Champlain. The uplift which produced the latter may have been the final cause for the disappearance of the former. If the Hudson water body survived long after the inauguration of Higher Glacial Lake Champlain, then deposits made by the outlet stream from that lake would be expected at the point where it debouched into the Hudson water body. They may be present, but the region where they would be expected has not been studied enough to determine this point. Some stages in the lowering of the Hudson water body are represented by the low-level deltas mentioned at South Bethlehem, on the Hoosick River, on the Batten Kill, on the Hudson River, and possibly on other streams. It is a question whether any of these fall within Higher Glacial Lake Champlain time. Possibly the 280–300-foot Hudson River delta between Glens Falls and Fort Edward does. If neither this nor the Oniskethau low-level delta at South Bethlehem falls within that time, then certainly the Hudson water body had been reduced to a very shallow representative of its former extent, for the latter delta is only 20–40 feet higher than the lowest part of the floor opposite this place.

Whatever may have been the history soon after the inauguration of Higher Glacial Lake Champlain, it is certain that long before the close of that history the Hudson water body had disappeared, and that the outlet stream of Higher Glacial Lake Champlain, the greater part of which flowed through the present Hudson River valley (see Fig. 23), had taken its course across the old floor of the Hudson water body, that the streams which had debouched into the

Hudson water body had extended their courses across its old floor to the main stream flowing through the bottom of the trough made by the meeting of the slopes of the old floor. Into this old floor the outlet stream of Higher Glacial Lake Champlain trenched its course at a rate so rapid that the tributary streams were unable to keep pace with it, and they were thus made to descend to their main over steep slopes, which the small streams have not yet succeeded in pushing back far from the present Hudson bluffs (see Fig. 16, *A*).

Where the mouth of the Hudson was at this time is a subject for discussion, but it is certain that the land was higher than now and that the Hudson, at least in those regions where it is bordered by clay plain, cut its channel to depths now covered by the waters of the Hudson estuary 10–50 feet deep north of Catskill. Just how deep the cutting of this channel in the lower Hudson was during Higher Glacial Lake Champlain time is a matter of less certainty for two reasons: First, because there has been subsequent filling, as shown by at least 25 feet of clay at Croton, which contains "flags" and shells, while the clay below does not contain them, as reported by the dredgers excavating clay from the river for brick-making; second, because of the occurrence of certain "deeps," the origin of which is a matter of discussion. Such deeps are the New Hamburg "deep" (120 feet), the West Point deep (216 feet), Stony Point-Verplanck's Point deep (102 feet), Fort Washington deep (155 feet), and others. These deeps may be due either to scouring[1] by the Hudson during Higher Glacial Lake Champlain time or by the tide since then, or they were original depressions bridged by buried masses of stagnant ice over which the large amount of clay eroded from the upper Hudson during Higher Glacial Lake Champlain time was carried to the sea. If such ice-bridges existed, the deposition of materials carried by the waters of the stream would not be necessary. Subsequent melting of the ice would leave the "deeps."

While the origin of these deeps is open to discussion, on the whole it seems certain that the Hudson had cut its channel to a considerable depth below present sea-level before the close of Higher Glacial Lake Champlain time. This necessitates an altitude of the land at that time higher by the amount of the general cutting, at least.

[1] For ability of a stream to scour its channel below sea-level see CHAMBERLIN AND SALISBURY'S *Geology*, Vol. I, pp. 162 and 184.

In the process of down-cutting the river terraces which occur in the upper Hudson and on the tributary streams in both upper and lower Hudson, were made. Some river terraces had also been made in the tributary valleys before the close of the history of the Hudson water body.

Before the close of Higher Glacial Lake Champlain history, it is believed, the depression which has drowned the lower Hudson and its tributaries had begun. The basis of this belief is the amount of filling of the southern Hudson since the submergence. While this is a matter subject to revision on more accurate knowledge, calculations made indicate that the contributions of the Hudson and its tributaries since the submergence would be inadequate to furnish the material for this filling, and therefore that some of it was supplied by the cutting of the trench in the old lake-floor or old sea-floor in the northern Hudson Valley, before the Fort Edward outlet was abandoned.

POST-HUDSON-CHAMPLAIN CHANGES OF DRAINAGE IN THE HUDSON VALLEY.

By the close of Higher Glacial Lake Champlain history nearly all the cutting by the Hudson south of Fort Edward had been accomplished. This is shown by the fact that the Fort Edward outlet floor is within less than 20 feet of the present Hudson level.

Aside from the trenching of the consequent courses of the streams below the floor of the Hudson water body, the pushing back of the steep gradients from the neighborhood of the Hudson River bluffs, and the development of subsequent tributaries on the consequent streams, a part of which at least was accomplished in Higher Glacial Lake Champlain time, there have been few changes in the valleys of the small streams since the disappearance of the Hudson water body. Depression of the land, part of which probably took place before the close of Higher Glacial Lake Champlain time, has drowned the lower courses of many tributaries from Troy south, and gravel has been carried out from the higher land into the trenches in the clay, making, in some cases, a gravel-floor, and in others, by further erosion, a gravel-floor and gravel-capped clay terraces, as on the Oniskethau. In a few places it seems likely that readjustments in drainage have taken

place because of the competition of neighboring streams. This seems to be true of the relations between Drummond Creek, a tributary of Saratoga Lake, and Outlet Creek, the outlet of Ballston Lake, which flows into Round Lake from the west (see Fig. 26).

Piracy of Outlet Creek and beheading of Drummond Creek.—Drummond Creek flows northeastward into Saratoga Lake through a rather broad, flat-bottomed valley, which is the northeastern part of one of the depressions between gravel plateaus south of Saratoga. Although this depression extends southwest beyond Ballston Lake, its southwestern part, including Ballston Lake, is not drained through Drummond Creek, but by a stream called Outlet Creek, flowing from Ballston Lake northeastward to a point a little over a mile from the lake near a place named East Line, where it makes a sharp turn southeastward and descends through a narrow, steep-sided valley with a high gradient to Round Lake (188 feet in altitude). At the point where Outlet Creek makes its southeastward turn its floor is something less than 280 feet in altitude. The question whether Drummond Creek has been beheaded by the working back of Outlet Creek, which tapped it and thus diverted its waters into Round Lake, would seem to rest upon the question whether Ballston Lake, which is now 285 feet above the sea, was ever enough higher to drain over the divide at 300 feet, down Drummond Creek into Saratoga Lake.

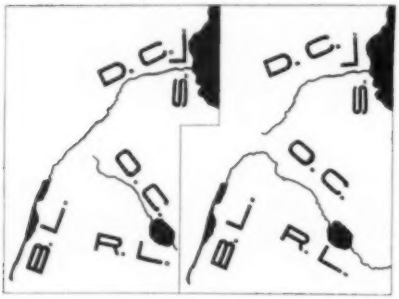

FIG. 26.—Piracy of Outlet Creek.

O. C.=Outlet Creek; D. C.=Drummond Creek; B. L.=Ballston Lake; R. L.=Round Lake; S. L.=Saratoga Lake.

CORRELATION OF TERRACES IN THE CHAMPLAIN VALLEY WITH THOSE IN THE HUDSON VALLEY.

In the description of the wave-wrought terraces of the Lake Champlain region mention was made of the fact that in the upper series of terraces there is a range from the highest to the lowest of

about 200–220 feet at Street Road and Crown Point. This great range extends north of the latter point, but not so far north as the Bouquet River. From this river northward the range is from 75 to 100 feet, apparently increasing from south to north. The question arises at once: What is the explanation of the greater range of wave-wrought features at Street Road and Crown Point? Were they produced wholly in Higher Glacial Lake Champlain, or are part of them due to wave-action in the preceding Hudson-Champlain water body before the inauguration of Higher Glacial Lake Champlain? The decision of these questions depends on the correlation of the terraces in the Champlain Valley and the water-levels represented in the Hudson Valley. Since terraces have not been found in the narrow east and west passages which would connect the levels in the Hudson and Champlain regions, the possible correlation of the terraces in these regions must be covered by a series of assumptions which shall include the range of probable fact.

First assumption.—If the making of the gravel plateaus at Street Road and Crown Point be correlated with the emergence of the barrier at the south of the Fort Edward outlet, then the wave-wrought terraces of the upper series all fall within the history of Higher Glacial Lake Champlain and the greater range here might be due to the cutting down of the outlet and consequent fall of water-level, before the ice had retired far enough north to permit the making of these terraces in the northern Champlain valley. If this be the true explanation, it would require a total cutting of the Fort Edward outlet of 200–220 feet, during Higher Glacial Lake Champlain history, which is greater by 60–80 feet than any possible barrier which the present topography will permit. The second hypothesis to account for the greater range of wave-wrought terraces in southern Champlain, on the assumption that they were all made in the Higher Glacial Lake Champlain water body, is that during the history of this water body there were not only the conditions mentioned in hypothesis 1, but there was also a warping upward of this particular portion of the basin, in excess of the up-warping at the outlet, so as to produce the extra spread of terraces. On the most favorable assumption as to the original height of the barrier south of Fort Edward, this would require no less than 60–80 feet of up-warping at Street Road and Crown Point in excess of that at the outlet.

The emergence of the barrier assumed in this correlation requires a fall of the Hudson-Champlain water-level of 120 to 160 feet, while the ice was retiring from the neighborhood of the barrier south of Fort Edward to Street Road and Crown Point. If the Hudson-Champlain water body was an arm of the sea, this fall of water-level was due to uplift. The time necessary to produce this uplift was sufficient to permit the making of at least one secondary delta near Glens Falls on the Hudson—the 340-foot delta, and perhaps a second—the 280–300-foot delta east of the latter (see Fig. 18, p. 455). It is possible, however, that the 280–300-foot delta was made in the Higher Glacial Lake Champlain water body. It is a question whether this length of time was not more than that required for the ice to retreat to Street Road and Crown Point, and to make any deposits that are known between the deposits in the vicinity of Glens Falls and these points. If the time for the ice to retreat from its position near Glens Falls to Street Road is indicated by the time necessary to make the two secondary deltas of the Hudson River, it would seem that the ice retreat was excessively slow. If, on the other hand, the rate of retreat was similar to that in the lower Hudson, it would seem that the rate of uplift was excessive. There are, however, some indications that the history between the making of the glacial deposits in the vicinity of Glens Falls and those at Street Road and Crown Point was somewhat complicated, and, if so, there may have been time for the uplift mentioned during this history. If the Hudson-Champlain water body was a lake, the fall of the water-level which preceded the emergence of the barrier south of Fort Edward was in part due to the cutting of the Brooklyn Narrows outlet. The full amount of the cutting of this outlet, so far as known, is only 122 feet. If this be distributed among the sixteen or more halting-places between Brooklyn and Street Road, it would produce but a few feet of fall during the time of the retreat of the ice from the barrier south of Fort Edward to Street Road and Crown Point. Even if allowance be made for the increase in rate of cutting of the Narrows outlet on the accession of the waters of Lake Iroquois through the Rome outlet, the lowering of the water-level from this cause, while the ice was retreating from the barrier south of Fort Edward to Street Road, can have been only a small part of the total change in water-level produced by the

cutting of the Narrows outlet. It would therefore follow that a large part of the 120–160-foot fall of water-level required in order to permit the emergence of the barrier south of Fort Edward was due to uplift, and the above remarks concerning the rate of the uplift and of the ice retreat would apply.

The correlation above assumed has the advantage of being in accord with the facts which suggest the existence of a Higher Glacial Lake George during the retreat of the ice from north of Glens Falls to near Street Road, and the disadvantage of permitting the formation of wave-wrought terraces at Street Road and Crown Point in Higher Glacial Lake Champlain in the approximate neighborhood of the ice under conditions which are cited below as causes preventing the formation of such terraces in the Hudson-Champlain water body.

Second assumption.—If the upper levels at Street Road and Crown Point be correlated with levels above the barrier south of Fort Edward, then the Street Road gravel plateau, the Crown Point high-level deposits, and a part of the upper series of wave-wrought terraces at these places were formed in the Hudson-Champlain water body. The absence in northern Champlain of the upper levels in the upper series of these wave-wrought terraces may be ascribed to the presence of ice here while they were being formed in southern Champlain. Under this assumption the demands made by the post-Higher Glacial Lake Champlain uplift at Street Road and Crown Point, will possibly permit the correlation (1) of the highest Street Road terrace with the 389-foot Glens Falls level, and (2) certainly with the 340-foot level. If the first correlation be correct, it is fatal to the hypothetical Higher Glacial Lake George; or if the Higher Glacial Lake George be real and not hypothetical, then its existence is fatal to the first correlation, and probably also to the correlation of the 340-foot Glens Falls delta with the Street Road and Crown Point levels, although it is barely possible that the latter correlation is permissible, even though the Higher Glacial Lake George did exist. If some terraces of the upper series at Street Road and Crown Point be thus assigned to the action of the Hudson-Champlain waters, then it follows that when Higher Glacial Lake Champlain was inaugurated the ice was as far south as the delta of the Bouquet River, where the

great range of the upper series of terraces has not been found. But if the ice was present on the Bouquet River when Higher Glacial Lake Champlain was inaugurated, there was time for the cutting down of the outlet as it retreated northward, and thus it would be expected that the terraces at the Bouquet River would have a greater range than northward where the uppermost wave-wrought terrace was not made until after the uppermost Bouquet terrace was completed. The failure of the terraces to show a greater range at this point (latitude of the delta on the Bouquet River) than farther north would indicate, either that the outlet was being cut very slowly, or that the water-level was being maintained at the north in some way, as the ice retreated from the Bouquet deltas. It is possible that uplift of the outlet maintained the water-level, or even caused it to rise in the northern part of the basin as the ice retreated, thus causing as great a range of terraces farther north as on the Bouquet River. The fact that the upper wave-wrought terrace near Whallonsburg is 30 feet higher than the surface of the Bouquet deltas would suggest that this tipping northward had been more than sufficient to overcome the effect of the cutting of the outlet as the ice was retreating, and that the water-level had actually been made to rise higher than its level when these deltas were made in the presence of the ice. If this explanation be correct, then at the south only, where the uplift of the outlet end of the basin could not maintain the water-level on the sides of the basin, would the range of the terraces be such as the cutting of the outlet alone would produce. Later, when the water-level began to fall throughout the basin, the ice had retired north of the Saranac River, and the range of terraces here seems to indicate that a differential northern uplift had begun.

Another interpretation.—The above has been written on the assumption that either the 580-foot Ausable, or the 640-foot Saranac gravel plateau, or both of them, are deltas made in the presence of the ice, and that the 500-foot Ausable and 520–540-foot Saranac deltas are the later product of erosion of the higher gravels. If it should be found that neither the 580-foot Ausable nor the 640-foot Saranac deposits are glacial deltas, but that the 500-foot Ausable and the 520–540-foot Saranac deltas are the highest, and if also some of the

more indistinct and uncertain terraces at Street Road and Crown Point be assumed not to be wave-wrought, then, because the wave-wrought terrace curve and the delta curve would be made to cross in the southern Champlain region, another succession of events must be assumed: After the ice had retired beyond the Saranac River, and after the 500-foot Ausable and the 520–540-foot Saranac deltas had been made in the Hudson-Champlain water body, the uplift at the south took place which inaugurated Higher Glacial Lake Champlain, and further uplift took place which tipped this water body into the northern end of the basin, causing it to rise to the level of the highest terrace in the upper series. The cutting of the outlet then permitted the upper series of terraces to be made.

On the whole it seems best to accept the reality of the upper terraces at Street Road and Crown Point, and to interpret these levels as in part Hudson-Champlain levels, and the lower part of this upper terrace series in the vicinity of Street Road and Crown Point and the entire upper series of terraces from the Bouquet River to north of the Saranac, as due to the waters of Higher Glacial Lake Champlain. While this may now seem to be the best interpretation, it certainly is not demonstrated.

ALTITUDE OF THE HUDSON WATER BODY.

If the Hudson water body was a lake, its height above sea-level is indicated by three things: (1) elevation of the southern barrier at that time; (2) height above sea-level of Lake Iroquois, which drained into this Hudson water body through the Rome outlet; (3) the amount of change in elevation of the barrier since it emerged from the Hudson water body, and produced Higher Glacial Lake Champlain.

Elevation above sea-level of the southern barrier.—If the submerged extra-morainic Hudson channel was used at this time, as a part of the outlet valley, and if the Narrows channel was cut entirely as an outlet channel, then the land must have been higher than now by 122 feet plus the amount of the slope of the channel to the sea. With the large volume of water flowing through this valley, it may have been cut to a very gentle gradient, and the elevation of the Hudson water body above the sea-level may not have been more than 35–50 feet. It may have been much more.

Evidence from the altitude of Lake Iroquois.—Since this lake drained into the Hudson water body, it follows that the Hudson water body was lower than Lake Iroquois. The level of the latter has been calculated at about 200 feet.[1] It follows then that the Hudson water body was less than 200 feet above sea-level, by the amount of fall of the outlet stream from Lake Iroquois to the Hudson water body. If the lower delta of the Mohawk described by Professor A. P. Brigham at Schenectady (340 feet A. T.) was deposited by the Mohawk and not by streams of ice-water from the ice-front,[2] it would require an average slope of this outlet stream of less than 2½ feet per mile to permit the Hudson water body to be above sea-level.

Amount of uplift of barrier south of Fort Edward since inauguration of Higher Glacial Lake Champlain.—Since the barrier is now no more than 260 and no less than 220 feet A. T. it follows either that the Hudson water body was above sea-level when the barrier emerged from it, or, if it was at sea-level, there has been an uplift of 220–260 feet since the inauguration of Higher Glacial Lake Champlain. Since changes in the gradient of the outlet valley require an uplift of 60 feet or so, since the close of Higher Glacial Lake Champlain history,[3] it leaves an uplift of 160–200 feet to take place during the history of this lake. If there was this amount of uplift during this time, then the Hudson water body was at sea-level when the barrier which produced Higher Glacial Lake Champlain emerged from its waters.

ORIGIN OF HUDSON WATER BODY.

There are two hypotheses to explain this water body.

1. (*a*) The water body was a lake made by a barrier at the south. (*b*) There was a succession of lakes made by a succession of barriers, or by a migrating barrier.

2. The water body was an arm of the sea.

Aside from the deposits made in its waters there are four series

[1] *Monograph 41*, U. S. Geological Survey, p. 775.

[2] *Bulletin of the Geological Society of America*, Vol. IX (1898), p. 205.

[3] This is based on the assumption that the valley at Whitehall has not changed in level and was 120 feet above sea-level when Higher Glacial Lake Champlain fell 120 feet to "Marine" Champlain level, and on a reasonable assumption as to the slope of this outlet valley.

of phenomena that must be accounted for in any explanation of the Hudson water body. They are: (1) the rise in level of the deltas and gravel plateaus northward; (2) the submerged channels, both in the lower Hudson and in the upper Hudson as far north as Troy; (3) the gap in the moraine at the Brooklyn Narrows, and the gap in the moraine at Perth Amboy, occupied by Arthur Kill (see Fig. 8, p. 426); (4) the scarcity of wave-wrought features.

1. *The rise of the gravel plateaus northward.*—Under either of the above hypotheses the land was relatively lower at the north during the presence of the water body than it is now, and there has been subsequent greater northern uplift. This greater northern uplift is necessary to account for the disappearance of the Hudson water body, if it was a lake, because the depth of the floor below the delta levels exceeds the known amount of the cutting of the outlet.

If the Hudson water body was a lake, the amount of northern uplift necessary to produce the present altitude of the deltas is greater than the amount necessary if they were formed in the sea, for the following reason: As the ice was retreating, the outlet was being lowered, so that the more northerly deltas must have been made at successively lower levels, unless there was some action to maintain the water-level. If the amount of cutting of the outlet be distributed among the sixteen or more different stands of the ice south of Street Road, it would cause but a small amount of fall between the successive stands. Even if the effect of the accession of the waters from Lake Iroquois by way of the Rome outlet be taken into consideration, and reasonable allowance be made for the increase in rate of cutting after that, it makes the fall in water-level between successive positions of the ice a comparatively small amount, much less than could be read from the topographic maps. Inequality in level of deltas due to this cause is much less than that due to unequal building up at the successive positions of the ice. In the aggregate, however, the amount of fall of water-level is considerable. If the cutting of the outlet during the history of the Hudson water body was 122 feet, it requires that the last delta made in this water body undergo an uplift of 122 feet more than would be required if they were all built in the sea at one stand of the land.

2. *Origin of the submerged channels.*—Under the first hypothesis

(the lake hypothesis) the land was not only relatively lower at the north than now, but at the south, from the beginning of the ice-retreat or soon after, it was higher than now. Its final altitude, however, before the recent submergence may not have been its altitude when the ice began its retreat. If the land at the south had its full altitude when the retreat of the ice began, then depression only is necessary here since then. If the full altitude was attained only after the ice had retreated some distance, the movement at the south was first one of uplift and finally one of depression. During the higher altitude, the channels, which are now under the waters of the Hudson estuary, were eroded and subsequent depression of the land has submerged them. Under the estuary or salt-water hypothesis the land was lower than now both north and south, when the gravel plateaus and other standing-water features were produced, and was subsequently uplifted; erosion produced the channels, and subsequent depression submerged them. Under either hypothesis, then, the retreat of the ice was followed by a time of higher altitude of land than now, and was succeeded by one of depression. The chief difference in the hypotheses is in the original altitude of the land and the time of uplift. According to the salt-water or estuarine hypothesis, the time of uplift was on the inauguration of Higher Glacial Lake Champlain, although the uplift may have been in progress in the southern Hudson before the emergence of the barrier in the northern Hudson that produced Higher Glacial Lake Champlain. The amount of this uplift before the disappearance of the Hudson water body is limited, however, by the levels which would give the sea access to the northern Hudson, if the water body was an arm of the sea. According to the lake hypothesis, the land was higher than now when the ice began its retreat, or soon after, and had either attained its full height then or did so during the retreat of the ice. According to either hypothesis, the full altitude of the southern Hudson had been attained before the close of Higher Glacial Lake Champlain history, and probably southern depression had begun.

3. *The gaps in the moraine.*—There are two gaps in the moraine between Brooklyn and Perth Amboy, the Narrows gap and the Arthur Kill gap (p. 426). The gap occupied by Arthur Kill has slopes which indicate that it may have been cut down from an elevation of from 25

to 40 feet above tide. The Narrows gap has steep slopes, which indicate that it may have been cut down from an elevation of 60 feet above tide. These estimates are not so reliable as they would be if the gaps were cut in a plain, because the moraine surface rises and falls, and a depression at a lower level than that indicated by the top of the steep valley side may have existed where these gaps now are.[1] However, it does not affect the results greatly whether the height of the barriers was a few feet more or less than the above estimates, but it is a matter of considerable importance to know whether the sea had access to the Hudson without an altitude of the land lower than the present, as the ice was retreating from the Brooklyn-Perth Amboy moraine. While it cannot be said to be demonstrable, the weight of the evidence seems to indicate that it did not.

According to the hypothesis that the Hudson water body was an estuary, these gaps must have been first scoured out when the land was enough lower to permit the sea to enter and the tide to scour. This requires a depression somewhat less, possibly, than 60 feet for the Narrows gap and 25–40 feet for the Arthur Kill gap. According to this hypothesis, tidal scour must have lowered these gaps to such an amount that, on the subsequent uplift which permitted the submerged channels to be carved out, either there was free passage for the streams that flowed through them, or they were scoured to a level lower than that of any other part of the barrier, and thus took off the drainage which finished the work of cutting away the barrier.

According to the hypothesis that the Hudson water body was a lake, these gaps were made by the outflow of fresh waters and not by tidal scour. This does not refer to the gaps at their present level, which may in part be due to tidal scour since the recent depression, but to their depth before the recent depression. If these gaps were cut by outflow of fresh waters, their relations are such as to require first a cutting as outlets of independent lakes, and later, when the ice had retired far enough to permit these independent water bodies to

[1] Professor Salisbury has suggested that these steep slopes may be due to recent wave-action. If this be the correct explanation, it makes the amount of cutting of the gap through the moraine even less certain. If, however, the overwash plain fronting the moraine was once continuous across the Narrows, as seems likely, the altitude of its inner margin (20–40 feet A. T.) marks the level from which the gap has been cut here.

coalesce, either (1) they did coalesce or (2) the outlets had been cut enough to so lower the water-level of each that they remained independent. If the lakes coalesced, then either (*a*) one of the outlets had been cut lower than the other, and thus rapidly drew off the water below the level of the other, or (*b*) both persisted and were rivals in the task of draining the lake.

In order that the requirements of the situation be clearly understood reference must be made to the maps showing the gaps and their relations to the present channels (see Fig. 8, p. 427, and Fig. 7, p. 425). From these maps it is observed that the Arthur Kill gap opens southward from Newark Bay, and that the Narrows gap opens southward from New York Bay. Newark Bay and New York Bay are connected by Kill van Kull ten miles north of Arthur Kill gap. New York Bay is connected with Long Island Sound by East River. The east end of Long Island Sound opens to the sea by wide channels. The land on the sides of both Kill van Kull and East River seems to indicate that the channels which they occupy have not been cut from much above sea-level. It follows, therefore, that if Arthur Kill and the Narrows gap were cut at first as outlets of independent lakes at the ice-front, and later as rival outlets of a water body that covered Newark Bay and New York Bay, they must either have been cut below the level of the divide between Long Island Sound and New York Bay before the ice retreated beyond it, or the present gap at the east end of Long Island must have been closed by a higher altitude of the land. Otherwise the Narrows gap at least would have been abandoned for a lower channel into Long Island Sound. While it is not at all unlikely that the gap at the east end of Long Island was closed, it is not essential for our present purpose to assume this. The writer, however, believes that the hypothesis that the present gap at the eastern end of Long Island was closed at that time by a greater altitude of the land is as tenable an hypothesis as any to account for the water body in which accumulated the recent clays of the Connecticut Valley and other valleys opening into Long Island Sound. It is in harmony with the published facts in regard to the distribution of those clays.[1] However, this is aside from the point under discussion.

[1] See N. S. SHALER, J. B. WOODWORTH, AND C. F. MARBUT, "Glacial Brick Clays of Rhode Island and Southeastern Massachusetts," *Seventeenth Annual Report*, U. S. Geological Survey (1895–96), pp. 957–1004.

If the Narrows gap and Arthur Kill gap were started as outlets of independent lakes at the ice-front, then by the time the ice had retreated far enough to permit these water bodies to coalesce, either both had been cut below the divide now crossed by Kill van Kull, and had thus produced independent water bodies in Newark Bay and New York Bay, or they became rival outlets to a common water body. If they became rivals, then one of the following things happened: One of them drew off the water below the level of the other, or before either one was victorious both had succeeded in cutting below the level of the land along Kill van Kull, and thus produced independent water bodies (in Newark Bay and New York Bay). If one was victorious, the Narrows gap, since it finally became the deeper, presumably was the one. Under this interpretation the Newark Bay Lake became tributary to Hudson Lake. Arthur Kill may have remained, however, the channel of the Rahway-Woodbridge Creek system (see Fig. 8), and thus have been deepened more. In either event, the Newark Bay water body became independent and drained either through Arthur Kill or by way of Kill van Kull. The deposits in the lowlands west of the Palisade Ridge were made in this independent water body. If Arthur Kill remained the outlet, then the present Kill van Kull channel is due either to tidal scour or to the work of a tributary working back from the Hudson, or to both. If Arthur Kill was abandoned, and Kill van Kull was the outlet of this Newark Bay Lake, the present channel is due to cutting by the outflow of its waters and to subsequent tidal scour, and Arthur Kill gap is due partly to cutting as an outlet of a lake, and later to cutting by the Rahway-Woodbridge Creek, and no doubt also to some recent tidal scour.

If the present channels be accepted as inheritances from the past, and not due to tidal scour, or at least not enough to obscure their former relations, it would seem that the Hackensack-Passaic system, along with Elizabeth River, finally became tributary to the Hudson through Kill van Kull on the disappearance of Newark Bay Lake, and that the Rahway River with Woodbridge Creek formed a system flowing through the Arthur Kill gap. If the submerged channel outside the moraine near Princess Bay Light, likewise is an inheritance from the past and not due wholly to tidal scour, the Rahway-Woodbridge

system was tributary to the Raritan, which, presumably, was tributary to the Hudson. The connection of the submerged Raritan and extra-morainic Hudson channel, however, cannot now be traced.

4. *Scarcity of wave-wrought features in the Hudson Valley.*—The almost complete absence of wave-wrought terraces in the area of the Hudson water body south of Glens Falls is not what would be expected. Even faintly developed terraces have been observed at a few places only. This apparent lack of effective wave-action may be due to the following:

(1) In the presence of the ice-sheet the water body was frozen over for considerable periods of the year, and during the summer season the presence of floating ice would tend to reduce the effectiveness of the wind in producing waves. This explanation would apply on either hypothesis for the origin of the Hudson water body. It would be more applicable if the body was a lake, but would apply if the water body was the sea, and if much freshened as suggested below (p. 650), it would be nearly on a par with the action in a lake.

(2) After the ice-sheet had retired to the northern part of the area, these conditions would no longer exist or would be much weakened in their effect and wave-action would be expected. (*a*) It is to be noted here, however, that the southern part of the area is the narrow part where the wind would have comparatively little chance to produce effective waves, even under the most favorable conditions of temperature. The greater width of the water body in the northern part of the Hudson would seem to necessitate effective wave-action after the ice retired into the Lake Champlain region and the climate had become warmer, so that the surface was no longer frozen over for so large a part of the year. Too much emphasis, however, must not be placed on the warming up of the climate, for boreal willows in the Salmon River section indicate a climate considerably colder than the present in Marine Champlain time. (*b*) It is to be noted, also, that this wide northern part of the Hudson water body was divided into smaller portions by numerous islands and shoals (see Fig. 13, p. 437), and these would decrease the efficiency of the wind in producing waves. (*c*) If this water body disappeared shortly after the ice had reached the Champlain valley, the length of time for this effective wave-action was reduced, and the earlier the dis-

appearance the more applicable the explanation becomes. If the Hudson water body existed until the ice had retired to the Bouquet River, or beyond the Saranac, as is considered in the various hypotheses stated for the time of origin of the Higher Glacial Lake Champlain, then there was a long time for the production of shore terraces. The time necessary to make the secondary deltas noted on the Batten Kill and the Hudson River would seem to be ample for the development of distinct wave-wrought terraces, and it is in this part of the valley only that features have been observed that may be assigned to wave-action, but it is surprising that they are not better developed here. (*d*) There is evidence that the water-level was not constant. Possibly there were two things to make it inconstant: first, the cutting of the outlet, and, second, crustal movement. The first could be true, of course, only if the Hudson water body was a lake. The second could be true under either hypothesis for the origin of the water body, and, as mentioned above, is necessary for the disappearance of the water body under either hypothesis.

Altogether the slight development of wave-wrought features in the Hudson is unexpected, and the above explanations do not seem to be wholly satisfactory, especially when it is recalled that under apparently very similar conditions distinct wave-wrought features were made in the Champlain region. The writer is forced to believe that a more detailed examination of the Hudson region will bring to light more evidence of wave-action.

Features unexplained by the salt-water hypothesis.—Certain features are present which are in accord with the lake hypothesis, but not with the salt-water hypothesis. There are certain features not present which seem to be required by the latter hypothesis, but not by the former. If the Hudson water body was an arm of the sea, the presence of some of these features and the absence of others must be accounted for. These features are: (1) absence of distinct wave-wrought features at the outer edge of the Brooklyn-Perth Amboy moraine at the levels required by the hypothesis; (2) presence of the overwash plain on Staten Island and Long Island without distinct features to be ascribed to wave-action; (3) entire absence of life certainly marine in the deposits made in the Hudson water body; (4) apparent absence of tidal distribution of muds in the Hudson

water body; (5) evidence of the altitude above sea-level of the Hudson water body.

1. Absence of distinct wave-wrought features at the outer edge of Brooklyn-Perth Amboy moraine.—There is an absence of wave-wrought features of a decisive character outside of the moraine in a region where the materials are soft and easily washed and which must have been exposed to strong waves from the Atlantic. Although these materials are displayed with a topography which would not offer the best opportunity for effective wave-action, yet it seems incredible that the sea could have been present over the area outside of the moraine, at the levels demanded by the gaps in the moraine and for the time necessary for the tide to scour out these gaps to the required depth, without leaving a decisive record in the easily eroded drift. Three suggestions aiming at an explanation of this are as follows: (*a*) That these gaps were first made by the wearing of ice-waters before depression took place, and were subsequently deepened by tidal scour when the land had been depressed enough to admit the sea. If this be admitted, the same early conditions as those under the lake hypothesis are assumed, the main difference being in the time of depression and in the number of depressions. (*b*) That ice protected the shore from wave-action. This would seem plausible for the time when, and the places where, the ice was present, but is difficult of acceptance after the ice-edge had retreated. Shore-ice might, however, have remained for long periods of the year, after the ice-sheet had retreated. (*c*) The land rose rapidly after the original depression, thus preventing the making of a distinct record of wave-action. If this was so, equally rapid scouring of the channels must be postulated in order to acccount for the access of the sea to the Hudson Valley. It is doubtful if the rapid rise would be effective unless the movement was very rapid, and then the scouring would be handicapped.

2. Presence of the overwash plains at the ice-front.—The presence of the overwash plains at the ice-front on Long Island and Staten Island without distinct features to be ascribed to wave-action, or a form that the presence of the sea over them would lead one to expect, argues strongly for an altitude of land above sea-level when the overwash plain was building. If the submergence hypothesis is tenable, it would seem necessary, as above, to postulate an altitude of land at

least as high as the present when these overwash plains were made, and higher than that when the gaps in the moraine were being cut, with enough subsequent depression to give the sea access, thus forming the Hudson water body. Such a crustal movement is the opposite of what would be expected as the ice retreats.

3. Absence of life certainly marine.—The only fossils that have been found in deposits in the Hudson water body are: (1) sponge spicules, fresh-water diatoms,[1] and worm tracks at Croton; and (2) leaves of *Vaccinia oxycoccus* at Albany.[1] No marine fossils have been found, unless the sponge spicules are such, and their identification, it seems, is uncertain. The presence of fresh-water diatoms is not necessarily fatal to the hypothesis of a salt Hudson water body, for they may have been brought into the salt water by the streams and deposited with the sediments in the salt water. If the sponge spicules are those of salt-water sponges, and if they were found in clays which antedate the recent depression, they settle the question of the origin of the Hudson water body.[2] Although there is an entire absence of life certainly marine in the deposits of this time throughout the entire stretch of 240–265 and possibly more than 300 miles through which the Hudson-Champlain water body extended (see Fig. 22), in the northern portion of the same region there is abundant evidence at low levels of marine life, which came up the St. Lawrence after the Hudson-Champlain water body had disappeared. Unless there is a sufficient explanation, this must be admitted as a strong argument against the salt-water hypothesis. However, it must be admitted that there is likewise a paucity of any forms of life in the Pleistocene deposits of the Hudson Valley. In explanation of the absence of marine life in this hypothetical long arm of the sea three

[1] See footnotes 3 and 4, p. 454.

[2] These sponge spicules were reported from Croton by Mr. Heinrich Ries in 1895. Since the above was written and first placed in the hands of the printer, word has been received from Mr. Ries that the sponge spicules are those of species not confined to salt water. The exact locality at Croton from which the specimens came is also a matter of some uncertainty. That they came from 20 feet below sea-level and were found in solid lumps of clay is certain, however. Inasmuch as some of the clay used at Croton for brick-making has accumulated in the Hudson estuary since the recent depression, it is possible that the clay in which these specimens were found was deposited long after the disappearance of the Hudson water body, and that therefore the fossils mentioned have no bearing on the origin of the Hudson water body.

suggestions have been made: (1) The waters were cold while the ice was near. (2) The waters were muddy while the ice was near. (3) The waters were freshened because of the great territory—at one time the Great Lakes drainage basin—drained into this water body, and because of the shallow sill over which little salt water could pass.

(1) In regard to the first suggestion it may be said that the waters were cold, but they were decreasingly cold as the ice retreated 240–300 miles and more northward. In Greenland, at the present time, marine life is abundant in the waters close to the ice-edge,[1] so that even if the waters were cold it does not appear to be a sufficient reason for the absence of salt-water life. Further than this, marine life invaded the Champlain region soon after the ice had retreated across the St. Lawrence and permitted the sea to enter. The fact that it failed to do so during the much longer time it had to get into the Hudson from the south while the ice was retreating northward argues strongly against the salt-water hypothesis.

(2) The waters were muddy. This argument would hold good so long as the ice was near. When at a greater distance, the argument does not hold good, for it seems to be true that there was comparatively little deposition of muds at a distance from the ice. Were it not so, the kames and other ice-molded forms at low levels would have been buried. The presence of marine life in the clays on the coast of Maine, and also in somewhat younger clays in the Lake Champlain region, indicates that marine life could exist while the deposition of considerable fine sediment was taking place. The explanation of the absence of life because of the muddiness of the waters therefore does not carry conviction with it, especially since there was so much opportunity for the introduction of life after the ice had retreated a great distance and the waters had become clear.

(3) The water was kept fresh because of the shallow sills over which the salt water had little access. This is perhaps the best explanation offered.[2] It necessitates a higher altitude at the east end of Long Island Sound than the present; otherwise there would

[1] Verbal communication of Professor Chamberlin. See also R. D. SALISBURY, *Glacial Geology of New Jersey*, p. 513.

[2] See R. D. SALISBURY, *Glacial Geology of New Jersey*, Final Report of the State Geologist, Vol. V, pp. 511–13.

have been abundant opportunity for life to come into the Hudson over the low land between Long Island Sound and the Hudson, and the Connecticut Valley deposits, which appear to resemble those of the Hudson in many respects would be expected to show abundant marine life.

It may be said, however, that this argument of shallow water over the sills has its limitations. It has been shown that it is necessary to believe that the channels through the moraine were cut down a considerable amount before the Hudson water body disappeared. On the submergence hypothesis tidal scour must be relied on to do this cutting, and reduction of the amount of water to get in over the sills at high tide reduces the amount that could go out with the ebb, and thereby proportionally reduces the efficiency of scour; and if none comes in, the scouring is reduced to the action of the fresh water supplied by the streams. This limitation would be more severe in Newark Bay perhaps than in the Hudson, where the great amount of fresh water flowing from Lake Iroquois into the Hudson waters after the Rome outlet came into use would be available for scouring the channel. The argument would not apply so well, however, in explanation of the absence of marine life in the Connecticut Valley. The demands of later events require a scouring out of the Hudson channel at the narrows to a considerable depth—possibly as much as 122 feet—before the Hudson water body disappeared. It would seem that this amount of scour would admit plenty of salt water and marine life. It may be, however, that water in the scoured channel was kept shallow by uplift, and the supply of salt water was thereby limited. Altogether it would seem possible that the explanation offered might account for the absence of marine life in the Hudson, if the delicate adjustment required to keep the water shallow over the sills existed. It is not known, however, that the conditions postulated actually did exist. The explanation would not apply to the phenomena in Long Island Sound, and in the Connecticut Valley, if the altitude of the land was as low as at present. It is doubtful if it is adequate even for the Hudson Valley phenomena without a higher altitude of land at the east end of Long Island Sound. This higher altitude is a requirement of the same kind, and nearly as great, as for the lake hypothesis in both the Hudson and Connecticut Valleys, and

greater than required for the Hudson Lake alone, which could have existed without this eastern uplift.

4. A fourth argument against the salt-water body is the absence of tidal action indicated by the fact that fine sediments were apparently not carried to any great distance from the ice-front. This is shown by a failure to bury some of the kames and other ice-molded features at low levels adjacent to higher gravel, sand, and clay. It may be said that this apparent failure of the fine sediments to be carried out on older and lower deposits in some situations, especially in the southern Hudson, is so extraordinary as to tax even the lake hypothesis. In some places this may be explained, however, by the persistence of protective stagnant ice-masses. There may be a question whether stagnant ice-masses would endure long enough to be thus effective. If there was sufficient tidal scour to cut the gaps in the moraine the amount required by later events, it is a question if the ice-masses could have endured long enough to prevent the burying of the kames, etc., by the finer sediments carried by the tide.

5. A fifth point bearing on the hypothesis that the Hudson water body was an arm of the sea is the evidence presented by its altitude when Higher Glacial Lake Champlain was separated from it. The evidence goes to show that its altitude at this time was something less than 200 feet above tide. How much less is unknown. If the amount of uplift while Higher Glacial Lake Champlain was being drained could be determined, the altitude of the Hudson water body when the barrier south of Fort Edward appeared would follow. (See pp. 639, 640.)

WHAT EVIDENCE IS THERE THAT THE HUDSON WATER BODY WAS A LAKE?

If the existence of a body of standing water be admitted, all the arguments against the submergence or salt-water hypothesis throw the scales in favor of the lake hypothesis. The evidence in favor of the lake hypothesis is as follows: (1) the existence of a barrier; (2) the evidence of deep channels cut through that barrier and submerged channels of drainage both inside and outside the barrier; (3, 4, 5, 6, 7) the five points mentioned above, as opposed to the salt-water hypothesis.

1. *The existence of a barrier makes the lake possible.*—Under the

sea hypothesis it also makes it necessary to explain the gaps in the barrier as, in part at least, due to tidal scour—an action which may have been limited (see the explanation for the absence of marine fossils as due to the shallowness of the water over the sills giving access to the salt water, p. 651). A barrier at the east end of Long Island Sound is not necessary for the existence of Lake Hudson, but an altitude of the land higher than now in that region is required by the supplementary explanation attached in this article to the salt-water hypothesis, and an altitude but little greater would produce a lake, and thus bring the Connecticut Valley phenomena and Hudson Valley phenomena under one explanation. It is not meant to imply here, however, that the Hudson and Connecticut water bodies were one water body. It seems certain that, if they were at an early date, they became independent later.

2. *The channels through the barrier and the submerged channels inside and outside the barrier.*—Under the lake hypothesis the outlet valley was a necessary feature, and the submerged channels are the natural consequences of the drainage of the lake, subsequent erosion by the Hudson, and later depression. Under the salt-water hypothesis the gaps must be explained as due to tidal scour which extended to a depth great enough to let the streams flow through them when elevation had taken place, but the completion of the channels was by the erosion of the Hudson at the subsequent higher stand of the land and perhaps by recent tidal scour. The lake hypothesis makes the gaps and extra-morainic channels now submerged, contemporaneous, in part at least, with the existence of the lake. The salt-water hypothesis makes them due in part to tidal scour, and in part to erosion following the uplift of the land and the consequent recession of the sea.

The channels inside the moraine in Newark Bay may be contemporaneous with the later history of the Hudson Lake. Under the salt-water hypothesis they follow uplift, but may be contemporaneous with deltas in the upper Hudson. The fact that the disappearance of the Newark Bay water body followed an uplift of the land of just about the amount necessary to cause this water body to disappear favors the submergence hypothesis (see p. 657).

ARGUMENTS OPPOSED TO THE HUDSON LAKE HYPOTHESIS.

There are two possible arguments against the lake hypothesis. One of them is based on the altitude of land farther south in New Jersey. Certain terraces on the south shore of Raritan Bay and south form in part the basis for belief in the lower altitude of land in that part of New Jersey, but such terraces are not found on the drift-covered north side of Raritan Bay.[1] This would indicate that the submergence which produced the terraces on the south side of Raritan Bay came earlier, and that, if it extended to the north side, emergence had taken place before the ice retired. Professor Salisbury assigns the date of the submergence which produced these terraces either to late glacial or post-glacial time,[2] but he considers the question of submergence in the vicinity of New York as still an open one.[3] The absence of distinct wave-wrought features on the Brooklyn-Perth Amboy moraine and on the overwash plain between Brooklyn and Perth Amboy does not favor the hypothesis of submergence there, and it is difficult to reconcile the absence of these features with the hypothesis of a lower altitude of land.

There is another objection to the lake hypothesis which, if it were valid, would argue for the salt-water hypothesis. It is that the southern barrier could not have lasted during the great time it took to make the clays in the region north of the moraine. As will be seen from the discussion of the origin of the gaps in the moraine, if the barrier consisted of the moraine only, and was limited to that part of the moraine above present sea-level, it must be admitted at once. It must be remembered, however, that at the time of maximum southern elevation the outlet stream was cutting through a wide stretch of land outside the moraine. As mentioned before, the shore line of this time may have been 95–100 miles farther south. It is very likely, also, that the outlet was never very high above sea-level, but that the uplift was taking place while the ice was retreating, so that the rate of cutting of the barrier would be kept close to a minimum. It must be remembered also that the character of the lower portions of the channels through the moraine is unknown. It may well be

[1] G. N. Knapp, verbal communication.

[2] *Glacial Geology of New Jersey*, p. 204.

[3] See *New York City Folio*, U. S. Geological Survey, p. 16.

that it is of such a nature as to resist erosion. It would be expected, indeed, that after a certain amount of erosion of the moraine the concentration of the larger bowlders which are common in moraines would form a pavement in the bottom of the channel and would check the down-cutting.

FIG. 27.—New York and vicinity as it would appear if depressed enough to permit the entrance of the sea over the probable original height of the barrier at the Narrows and at Perth Amboy, and if the depression south of the Raritan River were forty to fifty feet.

Black color indicates land not covered by waters during the hypothetical depression. The outline represents the present coast.

In conclusion, it may be stated that, while no single argument seems to be fatal to the salt-water hypothesis accounting for the

Hudson water body, unless those drawn from the phenomena on the outside of the moraine be such, it is likewise true that the facts are not fatal to the lake hypothesis, unless the sponge spicules reported from Croton represent salt-water species.[1] Aside from these sponge spicules, the weight of the evidence seems to be in favor of the lake hypothesis.

RELATION OF HUDSON WATER BODY TO THE CONNECTICUT VALLEY WATER BODY.

If the Hudson water body was an arm of the sea, there is no need of discussing the relation between the Connecticut Valley deposits and those of the Hudson more fully than they have already been discussed. It is enough to repeat here, what has been said before (p. 651), that in order to account for the absence of life certainly marine in the Hudson, on the hypothesis stated above (p. 650), it seems necessary to postulate a higher altitude of the land at that time at the east end of Long Island Sound, so as to shut out free access of salt water to both the Connecticut Valley and the Hudson Valley.

If the Hudson water body was a lake, it does not necessarily follow, of course, that the Connecticut Valley deposits accumulated in a lake. This explanation is given for these deposits in Massachusetts.[2] It is true nevertheless that a southern uplift somewhat more than that necessary to make Hudson Lake would equally well account for the phenomena of the Connecticut Valley. So far as published accounts indicate, there is little, if any, clay of late glacial age, outside of the area north of Long Island Sound, which could not be explained as having accumulated either in local lake basins, or in the sea when the land at the north was depressed enough to submerge the clay areas along the eastern New England coast. This northern depression is not incompatible with the southern uplift which would produce a lake in Long Island Sound and in the valleys and lowlands north of it. If Long Island and the land to the east were high enough to make a lake north of it, either from the start or later on, this water body was divided into several parts.

[1] See footnote, p. 649.

[2] See EMERSON, *Monograph XXIX*, U. S. Geological Survey, Chap. 19.

RELATION OF HUDSON WATER BODY TO WATER BODY WEST OF PALISADE RIDGE.

As already indicated, if the Hudson water body was an arm of the sea, so also were the waters in the lowland west of the Palisade Ridge by the time the ice had retired beyond the Sparkill Valley or earlier. If the Hudson water body was a lake, the waters west of the Palisade Ridge,[1] which may be called Newark Bay Lake, were probably independent while the ice was present and either drained through Arthur Kill, or first through that outlet and later into the Hudson by Kill van Kull. This Newark Bay Lake, no doubt, disappeared long before the Hudson Lake. It is interesting to note that when the ice had retreated beyond the Sparkill Valley, which crosses the Palisade Ridge just north of the New Jersey boundary (Fig. 9, No. 14, p. 429), the waters of this Newark Bay water body coalesced with those of the Hudson water body through this narrow valley, the bottom of which is now 20–30 feet above tide at the west side of the Palisade Ridge. Since the land was at this time down at the north by 75–90 feet more than at the south, it follows that this Sparkill Valley was the lower outlet, and that when the Hudson water body had been lowered to the level of this valley, the Newark Bay water body disappeared. Whatever cutting, therefore, was done at a southern outlet was accomplished before the ice had retired beyond this valley. It is likewise interesting to note that with this amount of northern depression the slope of the Hackensack Valley floor, for instance, would have been just the reverse of the present. If, when the water body disappeared this was true, the lower Hackensack, for instance, and other streams would have flowed in the reverse of their present direction and joined the Hudson water body through the Sparkill Valley. Since there is no evidence, so far as the writer is aware, that such a reversal has taken place, it would follow that by the time the Hudson water body disappeared there had been an uplift sufficient to produce a slope southward. That uplift must have been as much as 45–60 feet, and may have been more. Since the water-level must have been lowered this same amount in order to disclose the floor, it would seem that the disappearance of the water in this area was due to uplift. This

[1] For discussion of hypotheses to account for this water body see R. D. SALISBURY, *Glacial Geology of New Jersey*, pp. 195–200.

may have been true under either origin of the water body, and must have been true under the estuarine hypothesis. The early history of this water body was in part contemporaneous with that of Lake Passaic,[1] but the latter lake had disappeared before the Newark Bay water body had attained its greatest dimensions. When the Newark Bay water body disappeared, the floor was exposed as a broad stretch of plain partly covered with sand, through which the Passaic-Hackensack River took its course and was joined east of Shooter's Island by the extended course of Elizabeth River. From the sands of this Newark Bay lake-floor the dunes which occur on the west side of Newark Bay at various places were made.[2] If the peat under this sand is a salt marsh accumulation, as Professor George H. Cook thought, the interpretation must be altered accordingly.

RELATION OF HUDSON WATER BODY TO LAKE IROQUOIS.

The delta of the Mohawk River in the Hudson water body is reported at 340 feet above tide.[3] If this was made at a time when Lake Iroquois was draining out through the Rome outlet, it shows that the Hudson water body had a level lower than Lake Iroquois, by an amount, however, not necessarily the same as the present difference between the Lake Iroquois level and the delta level.

If Higher Glacial Lake Champlain was inaugurated before the ice retired beyond the Adirondacks, then here is the only opportunity to determine the relation between the levels of Lake Iroquois and the Hudson or Hudson-Champlain water body. If Higher Glacial Lake Champlain was not inaugurated until after the ice retired beyond the Adirondacks, then the waters of Lake Iroquois must have fallen to the level of Hudson-Champlain, and subsequently had the same level as that of Higher Glacial Lake Champlain on the uplift of the barrier south of Fort Edward. The weight of the evidence, however, is against this succession of events.

[1] See ROLLIN D. SALISBURY AND HENRY B. KUMMEL, "Lake Passaic: An Extinct Glacial Lake," *Annual Report of the State Geologist of New Jersey* for 1893, pp. 225–328.

[2] See *Geology of New Jersey*, 1868, p. 228, and *Annual Report of New Jersey State Geologist*, 1893, p. 205.

[3] A. P. BRIGHAM.

RELATION OF HIGHER GLACIAL LAKE CHAMPLAIN TO IROQUOIS.

Lake Iroquois was made by the ice blocking the St. Lawrence and causing the waters in the Ontario basin to overflow at the lowest point of the basin which was then near Rome, N. Y. During the retreat of the ice a differential uplift was in progress greater at the north. G. K. Gilbert says that when the Rome outlet (present level, 440 feet above tide) was abandoned at the close of the Iroquois epoch, "the water of the Ontario basin descended for a time along a course beginning near Covey Hill, and ending near West Chazy, N. Y."[1] Whether these levels are marked by shore terraces is not stated. If they are, it would seem that when the ice retired far enough north in the Champlain Valley, the waters of the Ontario and Champlain basins coalesced. This water body might properly be called Lake St. Lawrence—a name suggested by Upham in 1895.[2] If these levels are not marked by shore terraces, but simply by a series of outlet levels,[3] then it would seem that the waters of Higher Glacial Lake Champlain and the successor to Lake Iroquois did not coalesce, at any rate not until the close of Higher Glacial Lake Champlain time, when both fell to the levels which have been called "Marine" Champlain levels although the highest of these levels do not seem to contain marine fossils (see p. 626).

During "Marine" Champlain time these waters not only occupied the Champlain Valley, but extended into the Ontario basin, as is shown by the fact that the "marine" shores of the Champlain Valley extend westward through northern New York to the Ontario basin, being continuous with the so-called Oswego shore line.[4] In the Ontario basin as in the Champlain basin there is evidence of differential uplift greater at the north, in the late stages of the ice-retreat.

DURATION OF HUDSON WATER BODY.

If the Brooklyn-Perth Amboy moraine be correlated with the earliest Late Wisconsin terminal moraine, the history of the Hudson

[1] *Eighteenth Annual Report*, U. S. Geological Survey, Vol. I, p. 59.

[2] See *American Journal of Science*, Vol. CXLIX (1895), pp. 1–18; *Monograph XXV*, U. S. Geological Survey, p. 264. See also this article, p. 626.

[3] Since this was written and placed in the hands of the printer the writer has learned from conversation with Mr. Gilbert that this is the fact.

[4] G. K. GILBERT, *loc. cit.*

water body spans, or more than spans, the history of the entire system of moraines of that time represented in Ohio, Indiana, and Illinois. In terms of the history of the succession of the Great Lakes, it spans or more than spans, the history of Lakes Maumee, Whittlesey, Warren, Dana, and part of Iroquois,[1] and possibly all of the latter, according to the interpretation of the time of inauguration of Higher Glacial Lake Champlain. In terms of the history of Lake Chicago, it began earlier, and whether it ended earlier or later depends on the time the northern outlet of Lake Chicago was opened up by the retreat of the ice.

TIME DIVISIONS.

If the beginning of the retreat of the ice from the Brooklyn-Perth Amboy moraine be counted as Champlain time, then the time since the moraine was made may be divided as follows:

1. Hudson-Champlain.
2. Higher Glacial Lake Champlain.
3. "Marine" Champlain.
4. Present Lake Champlain.

Possibly the upper part of the levels marked "Marine" Champlain may represent another lake named by Upham Lake St. Lawrence, and thus make another division.

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[1] For an account of the history and relations of these lakes see LEVERETT, *Monograph* 41, U. S. Geological Survey, pp. 710-75.

Reviews

The Economic Resources of the Northern Black Hills. By J. D. Irving. With Contributions by S. F. Emmons and T. A. Jaggar, Jr. (Professional Paper No. 26, U. S. Geological Survey, 1904.) Pp. 222. 20 plates, 16 figures.

This report treats of the general geology and the economic or mining geology of an area of about six hundred square miles on the northeast flank of the great central dome of the Black Hills.

The essential structural features are: (1) the great central dome, now very greatly reduced and dissected by erosion, of laccolithic granite; (2) a border of greatly metamorphosed Algonkian sediments, which have been compressed into isoclinal folds, and whose strike is, in this district, northwesterly; (3) a great series of Paleozoic, Mesozoic, and Cenozoic sediments which dip away to the northward from the central igneous core. The central granite mass gave off great intrusive tongues which cut the Algonkian schists in all directions. Numerous later intrusions of porphyry are found in nearly all the formations from Algonkian to Benton.

On pp. 20 and 21 is to be found an excellent feature of this paper, namely, a summary, in tabulated form, of the essential points of interest in connection with each formation. Besides giving the usual facts needed for correlation, there is an additional column which is devoted to the essential topographic characteristics of each formation. From an examination of this, one can readily grasp how important a means in correlation the topographical element may become.

The chief interest in this region lies, of course, in the mineral wealth, and it is this phase of the subject to which the major part of the report is devoted. The main productive mining district is included in an area of about one hundred square miles, extending from the town of Perry, on Elk Creek, northwestward to the town of Carbonate, on the east bank of Spearfish Canyon, with its widest and most productive portion between Terry Peak on the southwest, and Gorden on the northeast. In the neighborhood of Terry Peak there has been the greatest igneous activity and also the greatest ore deposition.

The ore deposits are classified as follows: (*a*) those in the Algonkian;

(*b*) those in the Cambrian; (*c*) those in the Carboniferous; (*d*) those in eruptive rocks; and (*e*) those in rocks of recent formation.

The ore deposits in the Algonkian rocks include gold ores, copper ores, and tin ores. Of these the gold ores are the most important. The gold ores are also the chief ores in the region as a whole. The celebrated Homestake mines are working these ores.

However frequent the eruptive dikes and sills in the various formations, it cannot be said that the origin of the ores is due directly to the presence of igneous rocks, though they may be the ultimate source of the mineralizing solutions which made the deposits.

The ores in the Algonkian rocks occupy fractures and crushed portions in the schists, and are generally found to be richest beneath the overlying impervious shales of the Cambrian system. There seem to have been two principal periods of mineralization, one previous to the rhyolitic intrusions, and the other later.

The copper ores occur in small patches in the schists, and, so far as known, associated with graphitic schists, the graphite being supposed to have reduced the copper from the copper solutions.

The tin, in the form of cassiterite, occurs in two forms: (1) in pegmatitic granite, and (2) in placers. These deposits were never very extensively worked, and have been almost entirely given up since the collapse of the tin enterprise in the southern part of the hills. The tin is too much scattered thoughout the rocks ever to make it worth while to carry on extensive mining.

The ores in the Cambrian rocks are the ores of gold and silver in three forms, of which the second is the most important. These are: (1) gold-bearing conglomerates (fossil placers); (2) the refractory silicious ores; and (3) pyritous ores. Besides the gold and silver ores there are ores of tungsten, and lead and silver. The second group of gold and silver ores are called refractory because amalgamation has so far failed in their treatment, there being a great deal of secondary silica, pyrite, and fluorite in their constitution. They occur in channel-like bodies usually along the bedding planes of the dolomite, sometimes just beneath an impervious shale, and sometimes just below sills of igneous rock.

The ores in the Carboniferous limestone consist of two classes, neither of which is very important. They are likewise refractory, silicious ores of gold and silver, and ores of lead and silver. They were deposited, it is presumed, by ascending waters in fractures in the massive limestone, but without any particular concentration due to impervious beds, as in the earlier formations.

The ore deposits of later age are placer deposits along the numerous streams that head back in the central and older rocks of the region. Of all these the Deadwood placer is the richest.

The great number of admirable diagrams illustrating structure, and also the many excellent photomicrographs of the ores, are among the most valuable features of the report. The petrographic study of ore deposits is rightfully coming to command greater and greater attention from mining men as well as from scientists.

W. D. S.

Zinc and Lead Deposits of Northern Arkansas. (Professional Paper No. 24, U. S. Geological Survey, 1904.) By GEORGE I. ADAMS, assisted by A. H. PURDUE and E. F. BURCHARD. With a section on "The Determination and Correlation of the Formations" by E. O. ULRICH. Pp. 113, and 27 plates and maps.

THIS paper is a preliminary report rather than a final treatise on the district with which it deals. It represents a further prosecution of the study of the whole Ozark region wherein the same general principles of ore deposition will, doubtless, be found to prevail. It is very timely, as the scientific exploitation of the area cannot be said to have more than begun. The prospect holes greatly outnumber the productive workings, and the mines that are in operation lead a rather spasmodic existence.

The report deals with an area, comprising Marion county, the northern part of Searcy, the eastern border of Boone, and the northeastern part of Newton county, coming within the Yellville quadrangle, which lies in the much dissected plateau portion of the Ozark region, and is underlain by comparatively undisturbed sedimentaries that dip slightly southward toward the Boston Mountains.

These sedimentaries comprise Ordovician, Silurian, Devonian, and Carboniferous strata which have been moderately folded, fractured, and brecciated.

Ore was reported as occurring in this district by Schoolcraft as early as 1818. There are two ore horizons, the Yellville dolomite, Ordovician, the oldest formation exposed in the area, and the Boone chert of the Mississippian series. The ores are chiefly zinc blende and galena, but there is also a minor quantity of oxidized ore mined from the upper workings, and some zinc silicate, calamine.

The general movement of underground water, which was the medium by which the ores were concentrated, was essentially the same as that

which obtained in the Wisconsin and Missouri lead and zinc districts. In the opinion of the authors, however, there are some differences as compared with those regions. They believe that the metals were formerly disseminated chiefly in the Mississippian limestones, and that they were quite largely leached out of these and carried downward and deposited by downward circulation. The ores in the Boone chert formation are now found principally along fissures, while in the Yellville dolomite they are richest in the brecciated portions of the rock. Synclinal areas, on the whole, appear to be the most productive.

Secondary concentration by descending waters has certainly taken place; whether concentration by ascending waters has also taken place is not so clear. Messrs. Van Hise and Bain argue for an upward movement as the chief factor in the deposition of these ores.

Following the strictly geological part of the report is a more or less detailed description of the mines and prospects in the area, with excellent photographs and some valuable hints for the further exploration of the country.

The closing part of the paper is a rather general statement concerning the correlation of the formations of the region. As a number of fossil species, new to science, and many others not known outside of Arkansas, were found, a more detailed treatment of the faunas of this region will doubtless be forthcoming when Messrs. Ulrich and Girty shall have completed their studies of the material collected. The accompanying correlation table is a very helpful feature of the report.

It is to be hoped that the further opening up of the area will so facilitate the work of geologists that more details concerning the actual occurrences of ore and concerning structural relations will become known.

W. D. S.

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